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Encounters

Contemporary Art-Science Collaborations in the UK

PhD Thesis

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Abstract

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This thesis is a study of a group of scientists who, in the years after 1995, undertook collaborative work with artists. Although the interface between art and science has many historical aspects, they are not the main concern of this study. Attention is focused instead on what these contemporary scientists may have gained from collaborating with artists.

The recent growth of funding for such collaborations, for example from the Wellcome Trust, is discussed, and four projects are described. In a study of the relevant literature, links are found between contemporary work at the art-science interface and recent attempts to promote the public engagement with science. However the main theoretical drive of the thesis draws its structure from philosophical and sociological sources, principally the sociology of scientific knowledge (SSK).

In interview most scientists were explicit in their doubts that their artistic encounter could have any importance in their research life. Tacitly, however, they implied the opposite. The study finds several compelling cases of artists materially assisting scientific work. On a broader front, by examining the scientists' views on their professional culture, the thesis reveals that the scientists use the arts collaboration to explore views on a wide variety of metaphysical and professional issues. I argue that these explorations lead to changes in the way the scientists approach their professional life.

An important theme running through the thesis is the notion of ambivalence. Contradictions are found between the scientists' stated view of the method of science,

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and what they actually do. The scientists were also anxious to describe themselves as people possessing some autonomy. The thesis concludes by considering whether the art-science collaboration should be encouraged both for its creative potential, and for its ability to bring scientists into more reflective relationships with science, and with society.

Contents

Preface

This dissertation is an investigation of the contemporary art-science interface. The scope of the subject matter is huge and diverse and calls on a potentially vast sphere of reference. I am from the outset searching for a focus, anxious to anchor my work within a recognised disciplinary framework. The framework I choose is that provided by the philosophy of science and its recent manifestations, in particular the sociology of scientific knowledge (SSK).

The current study set its boundaries in two main ways. First it concentrated effort obtaining and analysing scientists' descriptions of their collaborations with artists. Secondly it persistently mined the philosophy of science and the sociology of science as the best sources of theory for interpreting those accounts.

Part of the interest of the project lay precisely in its potential unruliness. It would be possible perhaps to audit the last 10 years of UK-based art-science collaboration, and attempt some kind of reductive survey, revealing for instance the gender, age and discipline of the scientists, the practice of the artist, the type of product, and so on. This might be useful and interesting work, but it was not my preference.

How I came to situate this project forms the subject of the Prologue. Following David Silverman's suggestion (Silverman 2005, p305), this is a deliberately informal piece of writing. Quite early in the project I concluded there was neither an established background literature, nor an enduring 'canon' of art-science products, to root my initial studies. Instead I organised a round of encounters with people, books and exhibitions. I could not help but feel that the way my research questions were developing was alarmingly dependent on chance. Nevertheless a project did emerge and the Prologue explains how.

Acknowledgements

My sincere thanks go to my interviewees. They inspired the project and were unfailingly helpful and interested. By agreeing to be identified within the thesis they made my task of representing their ideas much easier.

My supervisors were Dr Jeff Thomas (Open University) and Dr Ken Arnold (The Wellcome Trust). From the beginning of the project they have combined kindly support with intellectual attentiveness. They have my profound and lasting thanks.

My employer Imperial College London has sponsored my studies, and has been generous in providing me time to pursue the research. In particular I would like to thank Dr Nick Russell, Director of the Science Communication Group at Imperial College. Dr Russell suggested I should undertake doctoral studies and was a steadfast advocate. Dr Felicity Mellor, my colleague at Imperial College, read the manuscript at a critical stage, and made valuable suggestions.

Especial thanks are due to my wife Giovanna and my sons Lorenzo and Luca. They have endured and encouraged a distracted scholar in their midst.

Chapter 1: Prologue

In 1999 the Wellcome Trust held a conference to discuss its Science on Stage and Screen awards scheme. I was invited to attend, and asked to prepare a short essay reporting on the issues raised (Webster 2000). With the heightened sense of awareness that comes with such a brief, I remember being alarmed at the very unsystematic nature of my reflections as the day went on. That is to say, although the conference had a single title, a single venue, and a single set of delegates, the various presentations and questions to my mind were entirely unbounded: synthesis seemed impossible.

At the time I was a secondary school science teacher in London. Though I had sat on a Wellcome Trust arts panel, and was moreover an invited observer and essayist, once in the conference hall I experienced a strong sense of alienation, of being in a world foreign to me. It was plain from the delegate list that the majority of the delegates were artists or curators. But it was not mere numbers that gave me my sense of estrangement. I felt I was not so much witnessing, as intruding upon, what I saw as the congenial exuberance of the arts. In my essay I wrote: "...the dominant crowd is artistic. The interesting scarves and good coats, slung so freely over the sombre seating, set the meeting apart from the average scientific event. That friendly hug over there; a theatre producer and an arts funder?" (Webster 2000, p7). When the talks and discussion got under way I saw that while all speakers were listened to politely, the most vigorous debate concerned the contemporary significance of art-science encounters. This was very different to the situation in scientific culture which I felt was marked by a vivid interest in the details, but inclined to be mute when the discussion became more general. I wrote: "Artists like to chat and argue; they flee the easel, the computer and the rehearsal room as soon as someone mentions a drink or offers money. Meanwhile, the scientists are bent over their benches and their grant applications, and virtuously ignore the glossy flyers from the Wellcome Trust, the Gulbenkian Foundation, and the Arts Council. Bless them." (Webster 2000, p9).

At the back of the hall were four scientists, sitting in line. They were like a Greek chorus, and their comments were spat out with some gracelessness. An earlier presentation, one of the scientists remarked, "reminds me very much of medical training films I saw 30 years ago". This was not intended as an interesting point about cultural continuity (the humanities habit), but as another way of saying "old-fashioned, dated, probably inaccurate" (the science habit).

Another of the four asked all the scientists in the room to identify themselves. Just a few hands went up. Everyone looked round, and at each other. Suddenly, dramatically, the absent scientists had become an issue in the conference hall. Even more dramatically a loud voice – it belonged to an actor – shouted out that they (the scientists) were all too busy to come to conferences like this: they were at work, of course. There was loud, persistent laughter, and in this uproar I sensed there was also relief. The relief arose, I concluded, because the delegates, perhaps not so relaxed in that wealthy and corporate Wellcome chamber, were recognizing a familiar idea . The thought was something like this: *"The scientists might be absent, but it is an absence we artists have got used to. Even at college, even back then, we never saw them. They were doing practicals all day, dissecting dogfish."*

The second experience was a meeting with Dr Claire Cohen, on the staff of the Kingston Business School, in March 2001. As someone interested both in collaborations and in arts evaluation, Dr Cohen had written a brief report on the Wellcome Trust's science-art scheme (Cohen 2000), and was preparing another (Cohen 2002). Cohen had interviewed some of the participating scientists, asking questions both about the management of the scheme, and about the experience of working with an artist. I interpreted Cohen as examining the collaborations from the perspective of the scientists, and as this was likely to be my approach too, I visited Kingston University to find out more.

Our conversation was discouraging. Dr Cohen emphasized what she saw as the particular difficulties of interviewing scientists. She had found scientists to be unforthcoming in discussing non-technical aspects of their work, for example the experience of collaborating with an artist. It was, said Dr Cohen, a problem of familiarity. Scientists just were not used to this way of talking and so it was difficult to draw out of them in-depth accounts of what their experiences had been. And so, I gathered, if my interest was in the scientists, the protagonists themselves might prove a resource of limited yield.

Talking with Dr Cohen, I was struck once more by the idea of "absence". Just as the artistic delegates at the Wellcome conference had giggled at a physical absenteeism, now I was learning of a mental absenteeism. Even if the scientist was before you, and being probed on his[1] collaboration, he might prove unresponsive, slow-witted, a poor interviewee.

In sum, these early informal enquiries presented me with puzzles. Had I not seen interest and enthusiasm among the scientists I'd talked to informally, up at the Wellcome Trust? Yet the scientists had been missing from that Wellcome conference, and, according to Claire Cohen, were anyway unrewarding - in a sense, still missing - when tracked down and taped.

Another puzzle concerned the question – I put the matter loosely – of benefit. Pending a more in-depth analysis of contemporary artistic culture, it didn't seem at all rash to suggest there were benefits to be had for the artists working in these collaborations. From artists' own assertions, to a recognition of the work taking place, to an audit of the largesse being deployed by funders such as Wellcome, the artistic pay-off seemed irrefutable. But for the scientists – my chosen group – I judged the matter less clear. I didn't have the sense, from my conversations with scientists, that 'scientific benefit' was a recognized feature, or goal of these projects. On the other hand, there they were, these busy and hard-pressed people, working away at the collaborations, and making time.

According to the cultural theorist Robert Darnton (Darnton 1985, p5), the stimulus for scholarly investigation can be a puzzle, a joke you don't get.[2] It was my puzzlement

[1] Throughout the thesis, for the purpose of simplicity, I use 'he' and 'his' to denote the third person singular. There is interesting research to be done on whether science-art collaborations are in fact gendered: in my experience the scientists have tended to be men, the artists women.
[2] Shapin (1995, p299) argues that disciplinary 'uneasiness' may be a sign that its practitioners are getting at the 'heart of the matter', and are addressing important matters.

about available accounts of science and art that gave me a starting point for my project. I came across statements which made the boldest of assertions about the special epistemic status of science and which seemed to rule out any possible role for art in scientific activity (Ede 2000, p32; Wolpert 2000).[3] Yet, scrutinized more closely, I felt the statements either made a tacit admission that the matter was not completely closed, or took so polemical a tone as to invite suspicion that the arguments defended a vulnerable position.

If these were some early encounters, what then would be my background theory, the model of enquiry to frame the investigation? At this time I was teaching a short course in the philosophy and sociology of science to science communication diploma students at Birkbeck College, London. It was hard not to be drawn to philosophical writings about the demarcation between science and non-science, or to the post-Kuhnian sociological tradition that sees scientific knowledge as thoroughly social in its genesis and development. Might not artistic practice, brought into so close a contact with a working scientist, take up position as one of those social 'causes'(Bloor 1976, p7) of scientific knowledge? For sure, so crude a model would need some elaboration before it could stand any chance of facilitating my enquiry. For example, while it seemed far-fetched to imagine the Edinburgh school[4] finding in contemporary art a plausible cause of scientific knowledge, had not David Bloor himself noted work connecting quantum mechanics to the politics of 1930s Germany (Bloor 1976, p7),

[3] Ferran (2006) and Leach (2006) are brief but sympathetic accounts of artists' relevance to scientific work. These articles survey art-science collaborations funded jointly by the Arts Council of England (ACE) and the Arts and Humanities Research Council (AHRC) in 2003.

[4] The 'Edinburgh school' (or 'strong programme) is the term for the tradition of sociology, most strongly associated with the work of Edinburgh university sociologists David Bloor and Barry Barnes. They argued (Bloor 1976) that all scientific knowledge, whatever its reputation, incorporates social causes

thus invoking ideas strongly resonant with the many texts that analyse relations between Einstein and Picasso, or Bohr and Cubism (Miller 1995)? And with demarcation being re-modelled as a sociological phenomenon dependent on 'boundary work', (Gieryn 1983) might I not also examine these collaborations as phenomena where scientists are actively exploring the boundaries of their professional identity?[5] Perhaps most pertinently, I had noticed in my casual conversations and pilot interviews that technical practice was a dominant theme among these collaborating artists and scientists. I was startled at the descriptions I was getting of artists and scientists who were quite literally working together (in laboratories, in zero-g aeroplanes, in galleries). To an extent, I was finding that artistic and scientific practice could make common cause. Surely I could find useful theoretical background in the rich vein of contemporary philosophy of science that was giving such importance to the concept of scientific practice (Hacking 1983; Rouse 1987, 1996; Pickering 1992, 1995; Buchwald 1995)?

No model is perfect. I was soon to find, as I read further, that the literature of the 'new' sociology of science is surprisingly cautious in its range. I had put hope in the sociology of scientific knowledge (SSK) for its reputation as a broker between wider cultural concerns and the daily pursuits of science. Yet it has often been pointed out that SSK is committed both to narrowly representationalist epistemological concerns (Shapin 1995, p315), and to the centrality of the laboratory as a site for sociological scrutiny (Knorr-Cetina 1983, p117). Each time I turned to the sociology literature, I was first lured by its interest in the daily practice of scientists (Gilbert and Mulkay

[5] Thomas Gieryn suggested that the divisions between disciplines depended not on established, pre-existing and natural differences between different sorts of fact, but on strenuous defence or invasion of academic territories (hence 'boundary work').

1982; Latour and Woolgar 1986) then disappointed by what I saw as the implication of these texts: that scientific practice is, after all, a wholly internal matter.

Further discussion of these issues I will hold over till later in the dissertation. Before leaving this brief 'natural history' of the genesis of my project, I will report one more encounter relevant for understanding the development of the project. I had been invited to give a seminar to Royal College of Art students on 'the nature of scientific research'. Inevitably my discussion of the subject made heavy reference to the way we might compare science and art, and I discussed my research. As with the 1999 Wellcome meeting described earlier (p1), I had a powerful feeling that I had over-estimated my ability to converse easily with arts professionals. At the end of the talk one of the students asked me a question that I simply couldn't answer, but which I knew even then was strongly relevant to my work. "Wasn't it a problem", the student asked, "that I was interpreting art-science collaborations as essentially romantic?" I had no answer. I wasn't even sure I knew a good way of defining romanticism. To the students however the thing was only too clear. My project had become infected by romanticism, and so was badly flawed.

The properly unromantic way to understand or create the art-science relation was to see it as a planned solution to a problem. I was told that problems like new computer interfaces, or the best way to model soundscapes in cities needed "planned input from both scientists and artists". And I had to admit that this was not my focus.

The charge of romanticism stayed in my mind right through the project. For it was true that I was indeed interpreting the meaning of these collaborations through the person of the scientist. As will become clear, the concept of the 'scientific self', the agent active in construction of the scientist's professional identity, was to become more and more significant in the way I interpreted the transcripts.[6]

[6] I use the term 'Romanticism' in the Wordsworthian sense that the proper understanding of any natural phenomenon requires an explicit recognition of a personality active in the apprehension of nature. Thus, when I refer to 'the scientific self' within this thesis, it is an attempt to describe the way the interviewed scientists traced the meaning of the collaborations not so much in terms of a final product, but in terms of their own personal experience.

Chapter 2: The Background

I. Introduction

Taking stock in 2002, an evaluation report from the Wellcome Trust concluded that the concept of science-art originated in the 1990s, and that by 2001 "... there was a discernable field of science-art both in terms of a community and a body of work" (Wellcome Trust 2002, p9).[7] The report included a number of excerpts from in-depth interviews and argued that the Trust, along with other organisations such as the Calouste Gulbenkian Foundation, could take credit for a new coherence to the concept of the art-science collaboration.

In order to elaborate further what might be meant by a 'field' of art-science, this chapter aims, first, to give background information about the contemporary art-science interface. Second, it describes four particular examples where a scientist has worked with an artist. The background information invites the reflection that, in the last 15 years, art-science collaboration has indeed become a significant affair, worth the attention of a study like mine. My examples are chosen because they display something of the range of the collaborations that I studied, and in their detail raise some of the issues that are consistently discussed in this thesis. In my concluding section to this chapter I summarise those issues, and point the reader also to the specific 'research questions' laid out at the end of my literature review.

[7] In this thesis the term 'science-art' or 'sciart' refers to projects where artists and scientists have worked together. The funding of such work, and its persistent presence in science centres and museums, only became systematic in the 1990s, due to decisions within the Medicine, Society and History division at the Wellcome Trust.

II. The contemporary art-science field.

In this section I sketch a possible outline of the contemporary art-science field. The events and relationships I describe are not so much a comprehensive list, as markers pointing to the field's vitality and breadth.

It is difficult to be decisive about those aspects of contemporary culture that will have enduring significance, a matter that exercises (for example) historians studying contemporary science and technology. In the same way that I consider it worthwhile to investigate an increase in institutional decisions that make new art-science collaborations or commissions possible, so the historiographer Thomas Söderqvist reminds us how interesting it is that more than 90 percent of all scientists that have ever existed have in fact been working after the end of World War II. To delay scholarly declarations about things that are going on now in science is to deny a historian's – or cultural theorist's – scrutiny of obviously significant developments: the rise of new fields (molecular biology), of new techniques (information technology), and the wider involvement of women (Söderqvist 1997, p2). Though the establishment of art-science collaborations as part of research councils' work has made only a tiny impact on their budget, the very fact that such streams have been instituted in recent years might surely attract scrutiny by historians and other scholars.

Yet the contemporary historiography of science presents particular methodological problems. Tracing significant patterns, and privileging or even identifying 'events' may paradoxically be hard when you are present yourself at that moment in historical time. According to Söqerqvist the problem is that there is on the one hand an absence of formal archives, and on the other, a superabundance of material in filing cabinets, on shelves, and on computer servers in the custody of individual scientists (Söderqvist 1997, p4). Relevant data is frighteningly ample because the due process of academic study, and the effects of time itself, have not yet produced the settled archive that allows the scholar to find his bearings. According to de Chadarevian (1997, p51) however the problem might be of quite a different kind. Material might be deemed personal and private, or subject to an institutional 30 year rule, or destroyed, or lost.

Hence the need for a cautionary note in speaking of a recent movement, or trend of art-science collaboration. Just to talk of the last 100 years, many 20th and 21st century artists and art movements have maintained an explicit interest in science and technology (Kepes 1956, 1965; Davis 1973). Vice versa there are scientists of the last 50 years who, in one way or another, have expressed an interest in the concepts and the practice of the arts (Waddington 1969, Pollock 1983, Frankel 2002). Given the existence of these particular flows of ideas running through modern culture, there is a risk of superficiality in arguing that, around 1995, a new movement was born.

Söderqvist notes that attempts to grasp the significance of contemporary cultural flows are likely to be drawn on the one hand to quantitative sorting of burgeoning quantities of data; while on the other exploiting the living actors to record their memories and even their actions, so as to draw up a rich and complex (but not necessarily comprehensive) narratival synthesis. Fuller gives a sense of just how 'rich' such an account might be when he describes a Söderqvist project thus: "the main product of [Söderqvist's 200 hours of interviewing] is not a mountain of facts but bonds of intimacy and trust that enable the historian to appreciate the scientist's existential ambivalence both in its own terms and as symptomatic of multiple social forces acting upon the scientist" (Fuller 1997, p246). At the same time Fuller points to the need for historians to find an appropriately subtle writing style capable of capturing such nuance.

'Existential ambivalence' - the scientist's equivocation on their role in their laboratory - might well be considered an important theme running through this dissertation. But I need first to establish some sense that the projects and people under consideration have a cultural density, or commonality, that allows them to be seen as a 'set' of ideas and working practices being worked through in contemporary artistic and scientific cultures. In what follows, therefore, I lay out quite briefly what might be called the 'land marks' of a possible chronology of the recent art-science phenomenon.[8]

At the centre of this discussion are the various funding decisions and grant calls co-ordinated by the recognised key player in the field, the Wellcome Trust. However, as argued elsewhere (Webster 2005), the involvement of agencies other than the

[8] My examples are drawn from the UK scene, but there are international analogues. Based in New York (USA) the agency 'Art and Science Collaborations, Inc.' aims to raise public awareness of art-science ventures, and to facilitate their work (http:www.asci.org, accessed December 5, 2007). The Exploratorium (San Francisco, USA) pioneered the use of art in science centres and labels itself "the museum of science, art and human perception" (http:www.exploratorium.edu/, accessed December 5, 2007). The University of Western Australia hosts 'SymbioticA', an "artistic laboratory dedicated to the research, learning and critique of life science" (http:www.symbiotica.uwa.edu.au, accessed December 5, 2007), trains artists in biological procedures. Most recent examples of cultural centres that display both art and science include the London venue Wellcome Collection and the Paris-based Le Laboratoire (Edwards 2007). Linda Dalrymple Henderson's wide-ranging survey of international events and publications exploring the art-science interface (Henderson 2004, pp424-428) is an authoritative summary. A steady source of information and intellectual comment on the art-science interface, in the UK and abroad, is the journal *Leonardo*, published by the MIT press. Although there are art-science collaborative ventures occurring in many countries, the UK seems to be unique in its possession of national funding streams for such arrangements.

Wellcome Trust make it possible to diagnose a significant degree of institutional legitimation of the field. The fact that art-science collaborations have been funded by organisations as different and as powerful as the Engineering and Physical Sciences Research Council (EPSRC), the Arts Council of England (ACE) and the WellcomeTrust suggests the art-science field has both utility and adaptability, qualities not associated with the ephemeral or short-lived. Thus, though my brief account has no designs to be fully comprehensive of the range of projects undertaken in recent years, nor attempts any demographic analysis, it does have at least one 'realist' ambition: to represent reliably the broad outlines of a significant cultural phenomenon.

The field's importance is often enhanced by linking it securely to a cultural shift of undisputed magnitude, namely the anxiety in government and scientific circles about public attitudes to science (Webster 2006).[9] Moreover it is clear from my own interviews that scientists engaged in collaboration with artists very frequently see public engagement as an important aspect of the work. The link is explicit in the Wellcome Trust's own literature (Arnold 2002), and forms a significant element of this study's analysis chapters. Indeed, it is an interesting point of discussion whether the art-science area can or should be separated from current debate about the public understanding of science. The extent to which these two zones of cultural debate have autonomy from each other is something that will be tested in the years to come. It is a judgement that will depend on the existence of research outcomes for scientists and artists, and on the contributions artists are found to have made to public debates of

[9] The House of Lords' select committee on science and technology produced a landmark report in 2000 that revealed a shift towards thinking that an 'engaged' public should be the aim of science communication (House of Lords 2000).

science. As discussed later[10], neither artists nor scientists speak with a single voice on the matter.

For current purposes, let us make 1993 our point of departure. This was the year the arts agency Arts Catalyst was set up with the aim to foster links between artists and scientists through commissions and strategic projects. In the same year a large permanent exhibition, 'Science for Life', was opened in the Wellcome Trust's London headquarters on the Euston Rd. The exhibition was a notable example of the way science museums were building into their design philosophy major commitments to interactivity. Subsequently Lawrence Smaje, Head of the Wellcome Trust's 'Medicine, Society and Health' division, began discussions within the Trust that led in 1996 to the setting up of a funding stream for art-science collaborations, the 'sciart'[11] scheme. Meanwhile, in 1994, the Arts Catalyst's first project, Talking of the Sex of Angels, was compiled with advice from scientists of Imperial College's Theoretical Physics Group.[12] There has been a strikingly broad and constant stream of Arts Catalyst projects every year since then (including the Zero Gravity project discussed in the current study by interviewees Nick Davey and Anthony Bull).

The Wellcome Trust's first Sciart awards were announced in May 1997: there were six winners, with grants ranging from £12,500 to £25,000; in 1998 another round was completed, again with six winners. By 2002 there had been a further four rounds of awards, with forty projects funded. Annual applications numbered around 150.

[10] See Chapter 6, sections VIII and IV.
[11] The terms 'sciart' and 'art-science collaboration' are often found to be used interchangeably. In this study, 'sciart' is used for the specific set of projects funded by the sciart Consortium, or by the Wellcome Trust. In general in this study I use the term 'art-science collaboration' to denote artists and scientists working together.
[12] http://www.artscatalyst.org/projects/archive/archive_talking.html. Accessed December 5, 2007.

During this time however Wellcome's science-art initiatives became themselves a collaboration, for the schemes were administered with other agencies as the so-called 'sciart Consortium', which ran from 1999-2002. Partners were the Calouste Gulbenkian Foundation, the Arts Council of England, the Scottish Arts Council, the British Council and the National Endowment for Science Technology and the Arts (NESTA). Spending was an annual £750,000. The consortium ceased to operate in 2002, with three rounds of grants achieved. There had been approximately 1500 applications, 124 awards, and a total spend of £3m (Candy 2007). Individual members of the consortium now continued with their own art-science funding, notably the Calouste Gulbenkian foundation.

The Wellcome Trust's commitment to the area had already been broadened by its setting up of another art-science scheme, 'Science on Stage and Screen', launched in 1998, and devoted more to performance and screen-based media. The Trust also funded a series of exhibitions in its own gallery space in London's Science Museum. 'Head On', the first of these exhibitions, was opened in 2002, with one of its installations the photographic result of the collaboration between Richard Wingate and Andrew Carnie, 'Magic Forest' (Albano et at., 2002).

The Wellcome evaluative report of 2002 (Wellcome Trust 2002), saw this period as marking some kind of maturity in the field. By now a real range of activities was discernable. Already mentioned are the collaborations and exhibitions sponsored by the Wellcome Trust. They also funded Marc Quinn's 'genomic portrait' of Sir John Sulston, unveiled in the National Portrait Gallery in 2001. But many other agencies too were setting up projects that could conveniently be labelled as sitting on the art-science interface. The London School of Hygiene and Tropical Medicine in 2001 set up its own series of art commissions and exhibitions (with funding from the Wellcome Trust and the Calouste Gulbenkian Foundation); the Cheltenham Festival of Science examined in 2003 a collaboration between the film maker Ken McMullen, the writer John Berger and the scientists of CERN.

This activity was accompanied by a stream of conferences and symposia organised separately by the Wellcome Trust, the Arts Council and the Calouste Gulbenkian Foundation, and jointly as a consortium (the conference hosted at the Royal Geographical Society in 2000, titled 'Partnerships in Science and Art', was the consortium's main focus of reflection on the activity it fostered). In 2001 The Royal College of Art and the Gulbenkian Foundation collaborated on a series of 'soirees' or conversations between artists and scientists, titled 'Science and Contemporary Art and Design'. Along with a great number of related activities – commissioned art works with a scientific theme, residencies in science institutes and in art centres, discussions at 'les café scientifiques', and even television broadcasts[13] - new strands of funding began to open up. In 2003 the Arts Council of England (ACE) and the Arts and Humanities Research Board (AHRB) began their own collaborative project, setting up a scheme for 'Arts and Science Research Fellowships'. These were explicitly collaborative ventures where an artist could win up to £35,000 grant for a project based in, and working with, a science institute. 16 such fellowships were announced in late 2003, and the scheme ran again in 2005.

[13] Blue Sky was a series of Open University/BBC broadcasts that looked at art-science collaborations, including that between Dr Nick Davey and Kitsou Dubois (http://www.open2.net/bluesky/dance.htm. Accessed June 14, 2007).

In 2004 the Engineering and Physical Sciences Research Council (EPSRC) set up a scheme of their own, fostering 'research networks' that set out from the beginning to include artists, designers and other arts practitioners in larger technical projects. With a starting budget of £500,000 this stream was pigeon-holed under the council's 'culture and creativity' strand and articulated the sense that diverse artists might bring important sets of skills to the work of scientists. Fourteen such networks were funded, each aiming to support research at the interface between science and engineering and the arts and humanities and to "demonstrate bi-directional *benefits to both* communities" (Engineering and Physical Sciences Research Council 2004, emphasis added). The research networks therefore fund meetings between groups of researchers – artists and scientists - seeking to work together on a project where an unusual variety of skills are seen to be necessary[14].

In all the above examples, it is the involvement of institutions that is being emphasised. The shape of the projects, or their quality, I leave aside. What seems remarkable is how many UK science institutions, to a greater or lesser extent, invited artists on to the premises, or gave them funding. In the years after 1995 arts residencies in science institutes became more common: they were set up at Imperial College London, at the Natural History Museum, at the Wellcome Genome Centre in Hinxton (Cambridge), at the Cambridge Cavendish Laboratory, at the Medical

[14] Project titles include: 'Interactivity, ubiquitous technology and music Performance', hosted by the High Performance Computing group at the University of East Anglia (£35,000); 'EngineeringArt: a network dedicated to exploring the art and science of materials', led by Dr Mark Miodownik at Kings College London (£41,500); 'Drama and performance for pleasurable personal learning environments' (DAPPLE), led by Professor P. Brna at the University of Glasgow (£38,500); and 'Culture and creativity: robotic feral public authoring', led by the curator Giles Lane (£23,500). http://gov.epsrc.ac.uk/ViewPanel.aspx?PanelID=3961&bannerlink=Panel%20Details. Accessed 17th March 17, 2005.

Research Council's National Institute for Medical Research at Millhill, and at the Science Museum, London.

It has become common to see works of art sited alongside science artefacts within science exhibition spaces. The web-based art-science agency SciCult curated an installation of Marilène Oliver's work at one of the Royal Society's 'celebration of science' in 2004 (Webster 2005). The Wellcome Trust has been a prolific presence in many of London's exhibition spaces, and elsewhere.[15] After the first exhibition at The Wellcome Trust's 'art' gallery in London's Science Museum there followed several more hosted: 'Metamorphing' in 2003, 'Pain in 2004 and 'Future Face' in 2005. These were successful ventures. 80,000 people visited Metamorphosis, 40% of them coming to the Science Museum specifically to see the exhibition (Arnold 2003). During this period the Trust set up temporary exhibitions in non-science venues too. 'Medicine Man', about the work and collections of Sir Henry Wellcome was an exhibition hosted by the British Museum in 2003. The exhibition 'Touch Me', a co-venture between the Victoria and Albert Museum and the Wellcome Trust, opened at the museum in June 2005. Most recently, with the Wellcome Trust's move into new headquarters, its building on 183 Euston Rd has been re-built to include Wellcome Collection, a series of spaces for exhibitions and events.

In a discussion whose task is to suggest the rapid propagation of art-science activity, the Wellcome Trust is likely to be a dominant voice. I have tried to show however that many other agencies have entered the field. It is relevant that the British

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[15] The Trust's touring science-art exhibition 'Wonderful: Visions of the Near Future' showed at the Cornerhouse, Manchester, the Arnolfini Gallery, Bristol, and Magma, Rotherham in 2004.

government too has seen importance in the area. The 2001 report *Imagination and Understanding,* produced by the Council for Science and Technology (which advises the Prime Minister), described the specialisation of the British education system as harmful, and went out of its way to argue that the Arts and Humanities Research Board (now Council), and the arts and humanities more generally, should come into relation with the sciences (Council for Science and Technology 2001, p1).

III. An overview of four projects

The above account attempts a broad overview of art-science activity, with especial emphasis on institutional investment. In what follows I shift the focus to a few collaborations I encountered during this study. Given that my aim is to convey a sense of what is meant by an art-science collaboration, a word is needed here about my selection criteria.

To my knowledge there is no published attempt to classify recent art-science collaborations or projects according to scientific discipline, art form, geographical location or other criteria. The overview I have just sketched, though focused on institutions, nevertheless points out the considerable diversity of the types of activity involved. The activities I chose to mention included arts residencies (for example Neil White's period at the Hinxton genome centre), portraiture (Marc Quinn's portrait of Sir John Sulston) and the photographic installation Magic Forest (made by the artist Andrew Carnie with assistance from the developmental neurologist Richard Wingate).

Just as an arts residency involves engagement with the staff of the host institution, so the portrait artist is necessarily attentive to his sitter (and in the case of Marc Quinn, the fact that the eventual portrait of Sulston was constructed from agar shows a serious interest in scientific processes). The scientists I chose to interview for this study I perceive as having been involved in a relation sufficiently constant, intense and productive to merit the word 'collaboration'. There is a point of terminology here. For while Marc Quinn's depiction of Sulston on an agar plate involved the artist spending time with Sulston, and involved some expertise in scientific procedures, the end product may not best be described as 'an art-science collaboration'.

Throughout this study I imply by the word 'collaboration' a relation more significant than we ordinarily associate with the act of sitting for a portrait. Similarly, I see an art-science collaboration as involving more than the transmission of information from the scientist to the artist (however important that might be). As will surely be seen as I progress through the analysis of the transcripts, these indeed are far from casual, quickly-forgotten encounters.

Nevertheless, while I am clear that the collaborations which interest me, and which I questioned the scientists about, are distinctly different from the relations implied by portraiture, or information-gathering, I prefer not to risk a simple definition. Reticence on this point is apparently common. A recent enquiry into the dynamics of international collaboration between scientists from S.Korea and the UK, notes "... a scarcity of definitions of collaboration", and complains that scholars have hardly looked at the dynamics of scientific collaboration, and how collaboration might impact on scientific practice (Hwang 2003). I attempt to remedy the situation somewhat, towards the end of the literature review, when I consider further the literature on scientific collaboration.[16]

Though the selection of the four collaborations below therefore follows no established classification, my criteria for their inclusion are clear. First they have much in common: each of the four projects gives what I believe is a good account of the way artists and scientists can work together closely on a common project. Second, though numbering only four, the projects are extremely diverse. In terms of outcome, 'Mapping Perception' led to a documentary and an exhibition, 'D'Alembert's Dream' a scripted animation, 'The Fluent Heart' a ballet, and 'Gravity Zero' research into posture maintenance. Third, they represent both the scientists' and the artists' voices: in the case of 'D'Alembert's Dream', it was the artist Phoebe von Held who was interviewed, rather than, as was usually the case, the scientist. Finally, they each in their own way point up themes explored more systematically in the later sections of the thesis. For example 'Mapping Perception' reveals the creative intensity of work that can mark the art-science collaboration, 'D'Alembert's Dream and Gravity Zero alerts us to the way obstacles and issues of communication are common too. 'The Fluent Heart', however, is a project the more extraordinary for the way it shows one person (Philip Kilner) who apparently embodies the diverse skills of artistic and scientific practice, and is able – happily for this project – to articulate the extent to which those practices converge in his one professional life as a cardiac imaging physician.

[16] See chapter 3, section IV (p96).

In the discussion that now follows, I not only describe these projects, but to an extent anticipate the analysis chapters by drawing on a very few excerpts from the transcripts. These serve to illustrate some of the themes that made the projects significant to the current study. Their fuller elucidation, however, is held over till later in the thesis.

a) Mapping Perception

At the centre of the 'Mapping Perception' project is an 11-year old girl, Eden Kötting. Eden suffers from Joubert's syndrome, a condition where the cerebellum remains underdeveloped, and leads to impaired mental function. Her father is Andrew Kötting, the independent film producer. During the long period of neonatal testing and diagnosis Kötting found himself reflecting both on how the medical world saw Eden, and how Eden saw herself.

The collaboration with the neurophysiologist Mark Lythgoe began in April 1999, and was curated by Giles Lane. In interviews and writings Lythgoe came to describe the work with Kötting as intense and life-changing. For a project that aimed to examine the scientific appraisal of Eden, challenging and supplementing this with the love of the parent, it was inevitable that part of the project would lie simply in the developing relationship between the scientist and the parent-artist. This was a collaboration that was also a cohabitation. Lythgoe was a frequent visitor at the Kötting household and came to know Eden well.

By the time I first interviewed Lythgoe the project was largely complete. A video was issued that year (2002), and an exhibition held in the art gallery in Southwark Park, London. Both elements attempted to contrast the scientific and human experience of malady and bring them into relation. In the Southwark exhibition for example one of the most powerful exhibits was a line of Eden's shoes, from the ones she wore as a toddler, to the most recent (Eden was 12 at the time of the exhibition). Visitors could see for themselves the steadily increasing size of the shoes and the scuffs and wear. They could interpret the apparent shifts in style as time goes by as the changing of Eden's taste in footware, or as reflecting the factor of contingency that jeopardises trips to shoe shops. Indeed, that long line of shoes – the most fraught part of any child's wardrobe – had precisely the effect of suggesting the essential normality of Eden.

How does science see the essence of Eden? Faced with quite marked neurological deficits at birth, the medical version of Eden was pessimistic and restricting. Kötting had obtained the medical records and excerpts were included in the film, running alongside film images taken of Eden as a baby: "Failure to progress – foetal distress – emergency caesarean section – seen by a neurologist – high pitched weak cry – global problems – we will see her again in one year". In the film Eden's verbal skills (but not her intellect) are shown to be compromised, but this inarticulacy seems to count for less and less as we watch the images of the Kötting household. Given the committed eye of the artist-parent, Eden becomes the normal child again. We see her cooking bacon and eggs; off-camera a voice calls: "Mmm, smells good".

This re-assertion of Eden's power, effected by the parents' love, seems at first sight an impossible ambition for the scientific forces that from time to time gaze in at Eden. In the film Kötting is at pains to include a great number of scientific terms and phrases, often voiced solemnly, as though liturgical. Both the terminology ("Eden has Joubert's Syndrome of Cereballar vermis hyperplasia") and the associated clinical recommendations ("Accurate assessment may be difficult at the start but is very important so the parents are not encouraged to aim too high") evoke a style of thought that is cautious. In the hands of the film maker, the scientific terminology is unsympathetic and remains free of any hopeful note. For the viewer then, depression and boredom mark out the filmic encounter with the scientific world, while the scenes of Eden growing up in the Kötting household have the opposite role of describing inspiration and pleasure.

Yet the film is the product of an art-science collaboration, and is far from being a diatribe against the medical world, or a polemic about its restricted vision. For though the medical comments about Eden are disturbing, they clearly represent only one tradition in medical working. We see Eden's ability to impact on her world mediated by various technical devices. As a toddler she has a hand-made harness that helps her smell and pick at flowers; later an adapted keyboard helps Eden propel herself into a world of words and conversation. Finally Eden herself is a commentator in the film. She describes her condition, using terms like 'genetic' and 'cerebellum', and frequently is seen holding a model of the brain and pointing to the cerebellum. It is noticeable that the intonations of the terminology are by an actor dressed in 18th century medical attire, as though such reductive imaginings are from the past, dead and gone.

The new world of science imaging, perhaps, will assist the medical world build a richer account of people like Eden. In the 'Mapping Perception' project, this is the possibility that Mark Lythgoe represents. Clearly Lythgoe has provided imagery, ideas and language for Kötting to work with. However Lythgoe's involvement extended far beyond emailing images and explanations from an office in the Institute of Child Health to Kötting's flat in Bethnal Green. According to Lythgoe, he "virtually moved in".

b) D'Alembert's Dream

Phoebe von Held is a theatre scholar and performance artist with a particular interest in the Enlightenment encyclopaedist Diderot. Her interest in an art-science collaboration was mediated by Simon Gould, an arts curator who worked for several years at the Medical Research Council's National Institute for Medical Research (NIMR).

Although Diderot is best known for his encyclopedia, he explored his interest in Enlightenment ideas in a number of different texts, including the fantasy *Le Rêve D'Alembert*. Here D'Alembert (a real character – he was Diderot's publisher) is dreaming aloud, all the time watched by an anxious land lady, Diderot and a physician. The dream is a vision of the future, and includes extraordinary accounts of phenomena that in modern discourse we would quickly identify as organic evolution, cloning, and the formation of chimera. Von Held wished to create a theatre piece where she dramatised the book, but incorporated actual speech from scientists working in the biosciences.

As the NIMR had an arts curator working in the building, the Mill Hill site was a good option for the artist to begin discussing Diderot with scientists. Von Held was advised about the staff likely to be interested and started setting up meetings. Her basic method was to send an English translation of *La Rêve D'Alembert,* ('D'Alembert's Dream')to a few scientists and ask them to read it. When that was done, Von Held would interview the scientist, asking them about their work and also about the way the scientist imagined future prospects.

These interviews were recorded and filmed and edited into a short pilot piece of about 10 minutes. In fact the film of the scientists was animated using rotoscoping, in which the outlines of (for example) a man talking are traced onto animation software. The scientists' accounts are contrasted with Diderot's words, as spoken by an actress in period costume.

According to Von Held, the scientists she worked with found themselves drawn to Diderot's text, and interested both in explaining their own research and in relating it to Diderot's imaginings:

> SW You asked them to partly think about 18th century ideas. Did you feel it was going to be a possibility to get them to enter this other world?
>
> PvH The references are very clear in the text. With Robin I wanted him to talk about cloning, they didn't have to make that so much of a leap, to go back. There were some who became very interested in the text itself, David

> Wilkinson for example, he went back and read the entire text, and read up on 18th century medical history on preformationism...[17]

In January 2006 some of the scientists Von Held had interviewed were once again filmed, but this time as actors. By this point a script was in development where the scientists' words would be more fully integrated into D'Alembert's Dream. Thus the artist, and a film crew, spent a day at Mill Hill filming the scientists as they acted parts of the script.

Yet though the scientists were interested and generous with their time, a key problem emerged in the collaboration that was hard to resolve but was explored at a script reading at a London University venue in November 2006. At this reading a possible script of the 'revised' 'D'Alembert's Dream' was tried out. There were actors to take the part of D'Alembert, his physician, and the voice of the 21st century scientist. Thus the original words of Diderot were being alternated with the descriptions given by scientists during interview.

In the feedback discussion afterwards a major theme was concern about the way the scientists' comments seemed so at odds with those of D'Alembert. The problem for some in the audience was that the Diderot text discussed the scientific advances of the future in a manner that was humorous, clever, and provoking. The scientific voice, by contrast, was heavily explanatory and passive, even though describing aspects of science that are ethically contentious, such as stem cell technology. While some in the audience professed themselves beguiled by the confident and technical assertions of

[17] This excerpt is discussed further in chapter 6 (excerpt 148).

the scientists, and construed them 'poetic', others argued that the stylistic mismatch between the literary and the scientific voice was jarring, and undermined the drama. The issue is perhaps best represented as a mismatch between a didactic, pedagogic style of scientific discourse, and a more open, playful and poetic literary discourse. The relevance to the current study is that time and again the scientists during the interviews referred to 'problems' they had in communicating with artists.[18] As it was not part of my method to observe conversations between artists and scientists, I have only the scientists' own accounts of those difficulties.[19] Von Held's experience of a chasm opening up between scientific and artistic discourse was to become a fruitful theme of exploration as I analysed the transcripts. For sure, I was to find in the transcripts evidence of gaps of understanding that proved hard to traverse.[20] Yet there were ample signs too of scientists being willing partners in joint attempts to convert the gap from barrier to trading zone.

Von Held had another problem especially relevant to the current study. One of her original ideas for the collaboration was to contrast Diderot's views about the future with the scientists' own perception of the world to come. It should not be surprising that a humanities scholar would not consider it too ambitious to ask scientists to discuss the future. Is it not a common cultural perception that scientists have a privileged access to the future? The famous comment by Snow that scientists "...have the future in their bones" (Snow 1998, p11), is reflected every day by media accounts of science, and by scientists themselves in their public discourse. The importance of developments in scientific knowledge is almost always interpreted through a

[18] See chapter 6, section VI.
[19] The issue of artist-scientist communication was also discussed by the artist Jane Prophet. See chapter 6, section IX.
[20] See excerpt 102, ch6.

utilitarian ethic, that is through the beneficial consequences to human or environmental health.

Von Held saw her scientists as having a tin ear for renderings of the future. Though articulate and captivating about their current work and its detailed significance, broader thoughts about the scientific future eluded them:

> SW So you had a goal in mind; that you wanted a technical conversation with the scientists about some of these 21st century concepts of evolution and mutation and cloning. Did you want to get a sense of what is going on now in terms of science?
>
> PvH Yes. I got the present world, but I was also hoping to get a sense of the future world which they might be projecting. Somehow this never materialised.
>
> SW They ...
>
> PvH It just never happened.
>
> SW Did you ask?
>
> PvH Of course. I asked at the beginning. Perhaps I could have probed more. And they are busy...[21]

[21] See transcript, p213 line 31 (Appendix 4).

Scientists being busy, or an artist being measured in her questioning, might not be the only explanations for the scientists' reticence in describing the world to come. The inhibition, at least in this collaboration, might be the scientists' entrenchment in their specialism:

> SW You admire their clarity when they talk about their work, but they won't speculate about the future, and they are uncomfortable about words like consciousness.[22] How do you understand that?
>
> PvH Well obviously it throws the responsibility back to the person asking the question. You have more and more sophisticated research agendas, more and more complex, which takes the responsibility from being a universal... they are very specialised in what they are doing; in respect to these big questions...
>
> SW Were you disappointed that they stuck to their expertise?
>
> PvH Not really because at the same time I felt I got so much out of them.[23]

The last two sentences demonstrate a theme commonly found in art-science collaborations. In this example an artist has approached a scientist with a proposal for a project. Firstly it is clear that the scientist has been identified as willing and able to discuss some scientific ideas that have also attracted the interest of the artist. Those

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[22] My question's reference to consciousness paraphrases conclusions asserted by Von Held earlier in the interview.

[23] See transcript p214, line 74 (Appendix 4).

ideas are the province of the scientist – they are his specialism.[24] There is a relatively comfortable zone where the artist elicits commentary from the scientist on his work, and is pleasantly surprised by the clarity and generosity the scientist displays, and his solicitous approach to the artist's interest. However, in the case under examination, there is resistance as soon as the artist requests that the couple move into wider cultural territory she signals as important to the artistic development of the project. Again the theme proves fruitful in the current study. For my interpretations of the interview material frequently invokes the phenomenon of ambivalence, or contradictoriness: time and again the scientist's discourse on art and science displays both a commitment to an isolationist epistemology, while leaving ample traces of serious attempts to end that isolation.

In this regard 'resistance' may not be the best word, for that would be to suggest a very active, articulate attitude to the artist's suggestions. No doubt such active, perhaps hostile resistance is sometimes the only result for artists making their first fumbling attempts to locate a sympathetic collaborator. An alternative possibility is to think of the failure to take up the artist's invitation as caused by a lack or a deficit, an interest either missing entirely, or embryonic and frozen. However the problem is to be construed, it resonates strongly with the methodological themes discussed later in this thesis, in particular those owing to Bruno Latour (1983, ch. 6), Rouse (1987, ch. 4) and Knorr-Cetina (1999, ch. 2). That is, the practices that make up laboratory life are particular and local and are productive only through the cultivation of a fierce rigidifying of method.

[24] In the current study I did not encounter a scientist who was working with an artist on a scientific theme outside their specialism.

Throughout this thesis I construe the scientists as using art to tinker with and perhaps ease that rigidity, both in terms of scientific process (eg Philip Kilner, excerpts 39-40) and professional organisation (Richard Wingate, excerpt 137). In the authors mentioned above the rigidity is described as extending outside the epistemic space of the laboratory, re-configuring social space in its own image. Indeed as described by Latour (1983) and by Rouse (1987), the only reason science can extend itself and apply itself outside the laboratory is because such a re-configuring takes place. Arguably such an interpretation has resonances with Phoebe von Held's experience. When recording the scientists describing their daily work, she was – temporarily – re-configured as a faithful scientific disciple and learner, and a highly productive interview resulted. As soon as she set the question about the wider future, von Held was effectively trying to reverse the configuring process, and found the scientists suddenly resistant. It is a good example of Rouse's (1987) Foucauldian interpretation of scientific practice being shaped by norms and by power relations. In my analysis chapter I shall enrich this concept of scientists' resistance to the 're-configuring' power of artists, by showing how that resistance is accompanied also by an active attempt to grant the artist power.

c) The Fluent Heart

In September 2003 Dr Philip Kilner won a 'Production' award from the Wellcome Trust, worth £124,500. The project, a collaboration between Kilner, the composer Sir John Tavener, and the choreographer Wayne McGregor, was the development of a dance piece on the theme of the heart. The resulting work, 'Amu', was premiered at Sadlers Wells, London on September 15th 2005.

The origins of the collaboration lie in Sir John Tavener's own heart condition, for which he was receiving treatment at the Royal Brompton Hospital. The site of Kilner's images of the beating heart put Tavener in mind of the musical rhythms of his own work, and of dance. Through conversations with his agent, Tavener was put in touch with Wayne McGregor, the lead choreographer and founder of Random Dance.

The details of the collaboration are at first sight straightforward, and reflect the working patterns of three world-class professionals: a composer, a cardiologist, and a choreographer. That is to say, they met from time to time, and in various ways. Wayne McGregor watched a heart operation at the Royal Brompton. Philip Kilner visited the Random Dance company and discussed the heart with the dancers. He also talked about the heart's rhythm with Sir John Tavener.

One reason for discussing the 'Fluent Heart' collaboration now is that this was an art-science collaboration different in style from that of the previous example, 'Mapping Perception'. This was a collaboration of partially intersecting professional trajectories, rather than the intense 'cohabitation' that marked out the work of Mark Lythgoe and Andrew Kötting. Another aspect of the collaboration worth noting is that Philip Kilner himself, far more than Sir John Tavener or Wayne McGregor, was to become the sole focus of my interest. Paradoxically, as I began to investigate the collaboration, the figure and the work of one participant, Philip Kilner, began to dominate. In

Kilner's professional practice, and through his unusual background (he had an arts, as well as a medical, training) I found a resource rich in experience of the art-science interface. In a sense, as I shall show in the interview analysis, Philip Kilner *is* an art-science collaboration.

A third point of comparison between this project, and 'Mapping Perception', is that the latter's products are surely to be construed as forcing reflection on a medical condition, as well as on Eden herself, and on the life and the qualities of her parents. The result of Philip Kilner's collaboration with Tavener and McGregor could certainly not be described as a work about the organ, or cardiology, or the experience of being a heart patient. 'Amu' is described by Random Dance as "... an inspiring musical dialogue of feeling, devotion and spirituality".[25] The company publicity on this web-site describes the piece as centred on the heart, but, if so, the grounding is more symbolic than literal. There are no projected images of the cardiac cycles for example, and like many of Sir John Tavener's pieces, is abstract and religious.

I was a member of the Wellcome Trust judging panel that adjudicated on the original application. Philip Kilner and Wayne McGregor were both present, and Sir John Tavener was involved via an audio conference link. Kilner started the presentation by describing his own work as a cardiologist. I felt that in his description of science's understanding of the heart's physical marvels, Kilner projected a strong sense that the fullest possible understanding of the heart would rely on a great number of skills, including those of the artist. This movement on Kilner's part towards the value of artistic engagement was then taken up by Wayne McGregor, the choreographer.

[25] http://www.randomdance.org/random/index.html. Accessed July 13, 2007.

Intriguingly, it was the artist who re-instated the physical sensibility that Kilner had eroded, by describing choreography as "like" the rhythmic movements of the heart.[26] There was then, between the doctor and the dancer, a dynamic where the former leant towards the non-material, and the latter towards the physical.

At this point the Chair of the panel, Professor Nancy Lane[27] leant towards the microphone and said: "Sir John, are you there? I wanted to ask you why you have become interested in science". Tavener described various aspects of the project but the most striking thing he said was "Nowadays, science is the new metaphysics". Tavener did not elaborate. Nor, unfortunately, did we ask. In the view of the judging panel however the three applicants presented a convincing sense that working together they would prepare something worth funding.

Later, in my methodology chapter, I discuss how I chose my interviews – how I 'sampled'.[28] In this case, it was the experience of the Wellcome board, in particular Kilner's obvious willingness to discuss his work with the interview panel, that made me think he should be one of my subjects. When, three months later, I was to interview Kilner in his office at the Royal Brompton, the interview touched on the collaboration hardly at all. From the start the interview focused entirely on his education, his training as a doctor, and his relation with clinical medicine:

> SW If we take the current "you", and the phenomenon of your story with Tavener and Wayne McGregor, could you tell me how that story began?

[26] I point here to the paradox of the artist taking an interest in mechanical matters, while the scientist emphasises the immeasurable complexity of his object of study (heart flow).
[27] Professor Lane researches neuroentomology at Cambridge University, UK.
[28] See chapter 4, section IV.

PK It begins [pause] I feel I should go back in my history much earlier.[29]

As a result of that initial decision by Kilner to excavate experiences occurring far earlier than any encounter with Tavener, the interview never 'caught up' with the 'Amu' project. Instead we explored Kilner's early disillusionment with the medical training, his retreat from professional life and his subsequent re-training as a sculptor at the Steiner-inspired art school, Emerson College. We also dealt with how he subsequently returned to medicine, and how his understanding of Goethe's philosophy, and of sculptural forms, came to inform his practice as an imaging cardiologist.

The relevant point is that although the interview never found its way back to the actual collaboration with Tavener and McGregor, it proved fruitful in exploring a point that the Wellcome panel members had felt so clearly: that Tavener, Kilner and McGregor were 'ready' to work together. The interview with Kilner brought out how important that 'readiness' is, and how difficult it might be to build. Indeed 'readiness' turned out to be a constant theme in the transcripts. For example, when I interviewed materials scientist Ian Thompson, and asked how it was that a research fellow in orthodontal surgery was able to take advantage of the skills of an artist, he explained the possibility of the relation not only in terms of the artist Paddy Hartley's useful sculptural skills (Hartley was his collaborator), but also in terms of his own rather unusual PhD supervisor, Professor Larry Hench:

[29] See transcript p142, line 5.

IT I'm slightly in a better position than some other researchers [because] Larry Hench my scientific mentor was always able to respond to different things. As a world leader of a certain subject you have to be always at the front and so you always have to respond to the slightest change in public opinion and scientific opinion [and so] whereas I do work sequentially towards a goal, I am slightly less blinkered than perhaps some of my other colleagues... because working with Larry is today we are doing this, but tomorrow we might be doing something completely different, because that is where the field suddenly verged to... My background with him, allowed me to spend some time sitting up from the trail of corn that we are following, and look around.[30]

Philip Kilner was an interviewee where the emerging interest for the current study lay less in the actual collaboration that I initially knew him by, and more in the 'collaborative' links Kilner was able to make – not without struggle – between different aspects of his own training and skills. In this brief overview of examples of science-art collaborations, Kilner's significance lies therefore in the way he attempts to transform his scientific practice not so much by collaborating with artists, as by attempting to integrate his artistic training into his professional, medical craft.

d) Gravity Zero

Gravity Zero was the name given to a project of great dramatic impact. The project was curated by Arts Catalyst, and was a collaboration between the scientists of the

[30] See transcript p177, line 199.

Imperial College Biodynamics Group, and the French dancer Kitsou Dubois. The climax of the work was a series of flights in a jet airliner adapted primarily for parabolic flights. These trajectories are the deep dives from high altitude that ensure the plane is safely flying, but is effectively in free fall. For 90 seconds everyone and everything inside will be freed from gravity, and will float about until the plane returns to its horizontal path.

Arts Catalyst has had for some years a relationship with the European Space Agency, most recently resulting in a commission to organise art project for the Agency's space station. The parabolic flights are used as training for space flights, and through the Arts Catalyst, a number of projects began to be put forward for inclusion on these flights. The project Gravity Zero involved two of the scientists interviewed in this study, Anthony Bull and Nick Davey. For Davey, it was the question of posture control, so remarkable in a dancer, that drew him to the collaboration.

Collaboration, and the logistics of shared projects, were defining features of Nick Davey's research. At Imperial College's Charing Cross site[31] Davey shared projects with psychologists and neurophysiologists. He also had a laboratory at the Stoke Mandeville Hospital – the National Spinal Injuries Centre – where he studied, and assisted with, rehabilitation. Other projects involving imaging were centred at Imperial's Hammersmith Hospital site; and at the time of the interview he was collaborating with the Biodynamics Group at the Imperial College main campus in South Kensington. These details are relevant because they provoke the question of

[31] At Imperial College London, both the Charing Cross and Hammersmith sites combine hospital, medical school and research premises.

whether the scientists capable of working with artists are already good at collaborating, and because they underscore the fact that the scientific life is highly 'distributed'. I shall be returning to the point later, when discussing the insights of science sociologist Karen Knorr-Cetina.

Nick Davey's art-science project must be seen as fitting into a complex web of multiplying collaborative projects. Davey was to describe the heavy implications of working like this. The working week was parcelled up into small fragments, each of them important, each of them routinely jeopardized: "I mean yesterday I was there and I made the mistake of checking my email ... so the work I planned to do out there I didn't do."[32]

I always felt it worthwhile asking scientists about their overall attitude to collaboration:

SW Are you drawn to collaborative work?

ND I think that a lot of the untapped areas of science are in areas between disciplines, and that's proved to be true sort of all the time in all my experience I suppose, and also it's quite fun doing that sort of work, it's quite fun doing something that's never been done before, rather than just extending... it's very safe to stay in your rut, and going on and doing the obvious next step, and my Head of Department doesn't altogether like that, but he said that provided I bring in grants, it doesn't matter, he doesn't mind. He

[32] See transcript p60, line 59.

has no real objection.[33]

The collaboration with Kitsou Dubois represented no break with Davey's daily research. In our interview transcripts, eight out of twenty pages are devoted to a discussion of Davey's science. The essential details are that Davey researched rehabilitation of victims of serious spinal injury, or stroke. In particular his interest was in the recovery of motor skills, including limb movement and posture. He was a pioneer in the technique of Transcranial Magnetic Stimulation (TMS), applying a device whose oscillating magnetic field could influence the behaviour of neural pathways both in the short and long term.

Davey's specific interest was in how a person's nervous system could compensate for traumatic injury. For example, if spinal injury had destroyed half of a set of motor neurones, could the remainder be made to function more effectively, and so return muscular stimulation nearer to its original, normal, level of excitation?

The collaboration with Kitsou Dubois depended on the fact that Dubois is a dancer. Dancers show in their movement shifts and control of posture quite impossible for the untrained body. If a normal person projects out an arm, thus shifting the body's centre of gravity, the trunk muscles in the abdomen automatically contract, twisting the body so as to adjust the posture to a stable relation to gravitational forces. Davey's interest in spinal injury had shown him that one of the problems in this type of trauma is a loss in the ability to control the muscles in the trunk, which are anyway quite difficult to control voluntarily. Could the processes he was studying in the motor excitation of

[33] See transcript p76, line 475.

muscles in the leg apply also to abdominal muscles? Davey saw Dubois as a 'natural' experiment in the possibility that trunk muscle control could be learnt. Trunk control that maintains balance is fundamental to the dancing skill:

> ND The beautiful thing about that is that you know it can be trained and learned. And in dance, you know I went to the Royal Ballet, my wife took me to the Royal Ballet for a birthday present, and I was watching, it was around the time that this was going on, and I was watching the dancers, and they do not move at all, their bodies – I could line their head up with their background, and they were making far more intricate movements than just sticking their arm out, you know standing on one leg, kicking their legs out, moving their arms out, even twisting their torso, and they didn't tilt at all, so something has trained them up, and it's more believable that you could train something cortically than you could train something reflexively...[34]

In Kitsou Dubois the adaptation was learned, because she had trained to be a dancer and make these kinds of movement. Davey thought he might be able to study Dubois to determine whether the adaptation occurred in inter-neurones in the brain, and thus was cortical, as well as reflexive. Meanwhile Davey had been establishing that TMS could have the parallel use of actually effecting changes in the behaviour of motor neurones. Here then was the sense of this art-science collaboration: by putting Dubois into zero gravity, and measuring the electrical basis of her posture control, any patterns specific to her could be assumed to be rooted in her ability to train the

[34] See transcript p68, line 205.

neuronal organisation and excitation levels in her motor cortex, rather than in any spinal reflex determined by gravity.

In zero-g Dubois could maintain her posture and go through a routine of moves. An ordinary untrained person, suddenly floating in free fall, goes entirely flaccid. For Dubois, and Arts Catalyst, this fact alone makes dancers a possibly important input in astronaut training. Davey had another interest. Through Dubois he would be able to show, neatly and quickly, that the control of trunk muscles is not merely a reflex response to gravity, can be learnt, and may be susceptible to the TMS technology he was developing.

To conclude this section's tentative tracing of 'types' of art-science collaboration, the significance of Gravity Zero lies in its clinical application. That is, this was an art-science collaboration where a clinical research value is very clear. Nothing less than an improved understanding and treatment of spinal trauma is promised. Yet there are also public engagement outcomes too. Nick Davey and the Open University made a TV broadcast about the project, and Davey confirmed in interview that the project attracted media interest. Kitsou Dubois herself made films of her free-fall dancing, and she developed choreography specific for these unusual situations.

I end this section by making a point about the difficulty of these collaborations. Very often art-science collaborations attract descriptors such as "creative", "fruitful" and "exciting". Collaborations within science are known however to pose significant problems of communication and trust (Hwang 2003, Hackett 2005). Problems of communication and of professional culture were frequently discussed by my interviewees.[35] In surveying the cardiologist Philip Kilner's practice, it became a significant point that effort was needed to sustain the relationship between his arts facility and his clinical practice. Though by his own account the link was both profound and useful, and therefore worth pursuing, there are signs in the interviews that finding ways in his daily work to use that link was not straightforward. Something of the same is evident with Nick Davey: the utility and interest of his collaboration with the dancer Kitsou Dubois is plainly described in the transcripts. But the fact that Dubois works in France, is a French speaker, and is an artist rather than a biologist or clinician, poses challenges. According to Davey: "I mean we've got discipline problems, we've got language problems, we've got different sex problems as well". Davey's interview was intriguing for the way it showed as much conflict in what the partners wanted and believed, as it revealed a pooling of skills and questions for mutual investigation. Gravity Zero, the final project of this over-view, points therefore to the need to consider the obstacles that threaten to block the work of the collaboration. There is interest to be had from considering the ways various factors might shape or impede an art-science collaboration, such as the logistics and institutional issues voiced for example by Anthony Bull,[36] and the tacit epistemological commitments articulated by Richard Wingate.[37]

[35] See chapter 6, section VI and VII.
[36] See excerpt 118, chapter 6.
[37] See for example Richard Wingate's comments, excerpt 6, chapter 5.

Chapter 3: The Literature Review

I. Introduction

This bibliographic chapter has two general aims and encounters one general difficulty. The first aim is to give a sense of the great range of ideas associated with enquiries into art-science relations. The second, contrasting, aim is to assess this literature in a manner that allows my research questions to narrow down to a clear focus. The general difficulty is the potential size, and the variety of styles, of the background literature. I start this review therefore by briefly exploring its scope, and mention a few key sources. I then embark on a detailed analysis of a number of texts I consider particularly useful to the development of my research questions.

In a review of Kemp (2000), Montgomery asserts that there now exists a recognised area called art-science studies, which he described as "fecund and weedy" (Montgomery 2004). As Montgomery implies, the topic is interdisciplinary and inclined to ramify in all directions. Yet it is easy enough to set out a few zones that have so far proved particularly fruitful sources of ideas. Within the history of art, for example, the field of Renaissance studies is much preoccupied with the question of how best to characterise the work of such headline figures as Leonardo da Vinci, Galileo Galilei, Filippo Brunelleschi, Christopher Wren and Robert Hooke (Panofsky 1956;Kemp 1990; Jardine 1999, 2003). These broadly biographical and historical texts are complemented by work that explores certain consequences of Renaissance skills, such as the development of perspective in painting (Kemp 1990), or the use of optical devices by artists (Hockney 2001). Another much-studied area, one perhaps more promising as background to my own study of a portion of contemporary culture, centres on intellectual development in early 20th century Europe. At the time that Picasso and his associates were developing the art theories of Cubism, and decisively accelerating the move away from purely figurative art, science itself was taking on the concepts we associate with Albert Einstein's theory of relativity, and with quantum mechanics. The considerable literature that comments on the significance of this contemporaneity is far from united. There are diverse views on how best to study or theorise possible connections between Einstein and Picasso, or Bohr and Cubism (Henderson 1983, 2004; Vargish and Mook 1999), and it seems, how best to publish these views. For in addition to work published in peer-reviewed academic journals, some writers on this topic seem to be pushing towards trade popular science or biography (Miller 1996, 2001), while others have compiled large illustrated volumes of the 'coffee table' style (Waddington 1969; Gamwell 2002). In terms of the current study, my interest in these texts is less in their conclusions about Picasso's indebtedness to Einstein as in the nature of the authors' struggles, more or less explicit, to find a method and a terminology for describing those relations. Some texts, of course, see such struggles as inviting reflection on the ways theorists have traditionally approached the art-science interface or 'binary economy' (Jones and Galison 1998 p2).

The bulk of academic interest in art-science relations exists in the disciplines that focus on the history of art and the history of science (Topper and Holloway 1980). The philosophy of science, traditionally the area that rules on the nature of the scientific process has not so far turned its attention precisely to the art-science interface (a striking exception is Gere 2004). However one way to push the issue of art-science relations into the orbit of the philosophy of science would be to note the latter's historic interest in the nature and importance of disciplinary boundaries between science and non-science. This is the force of Kuhn (1977, ch.14), which reflects on art and science, and which I will discuss in the main body of this review; but in the context of mid-20th century philosophy of science we associate demarcation issues particularly with Karl Popper (Popper 1959, 1972).[38] More recently however, philosophical debate on disciplinary boundaries has shifted decisively away from the epistemic rules of demarcation, to the ways boundaries are socially negotiated (Gieryn 1983, 1999). In such debates the fragmentation of science is based not so much on diverse commitments to different portions of nature, as on the existence of locally developed practices and habits, and on expertises that are transferable only with the greatest difficulty (Latour 1983).

Gieryn's work emphasises the busily social processes that shore up, or erode, disciplinary boundaries. His call for the study of practice as a vital conceptual tool for understanding scientific development reflects of course one important trend in the philosophy of science's contemporary academic descendent, SSK. I will argue at various points in this thesis that though SSK resembles its parent in showing no great interest in the work and culture of artists, there are aspects of its work that are helpful to an analysis of contemporary art-science collaborations. If SSK has as its most fundamental stand a principled scepticism towards the possibility of a united and isolated science[39], then the phenomenon of artists and scientists developing practice together must surely be interesting. More precisely, if these collaborations reveal artists and scientists actually working together on lab-based projects, or on other aspects of a scientist's practice (such as grant proposals, or the marketing of the laboratory), then my study might draw from, and in turn inform, the ideas developed in numerous SSK texts about scientific culture and practice (Knorr Cetina 1981, 1999; Knorr-

[38] There is an important philosophical literature on the differences and similarities in the methods and goals of the natural sciences and the human sciences (Winch 1958; Hesse 1980; Galison 2000).
[39] Mulkay (1979, ch1) contains a detailed appraisal of 'separatist' epistemology and its relation to current trends.

Cetina and Mulkay 1983; Gilbert and Mulkay 1984; Latour and Woolgar 1986; Traweek 1988; Pickering 1993, 1995; Buchwald 1995).

My emphasis on SSK in both my reading and my methodology forces sparser use of other fields that comment regularly on the art and science interface. Most obviously, though later in this review I give a brief overview of selected texts from the field, this thesis is not a history of art. It refers to, but does not dwell upon, contributions from fields as diverse as aesthetics (Winterbourne 1981; Gideon 1990; Topper 1990,), psychology (Arnheim 1969), and digital art (Gere C. 2006; Wilson S. 2002). I have wanted this study of art-science collaborations to aid reflection on scientific practice, and scientists, and for that reason my background theory is dominated by material from science studies.

The above account gives the rough scope of a possible literature, and begins to account for my own particular choice of selected readings. I make clear that though a study of art-science collaborations is bound to be eclectic, I have been aware from the beginning of the risks of being drawn into an ambitious synthesis. I have prioritised the literature of the philosophy of science, and of the sociology of science, as sources of theory. Using these fields to help me get traction on my reading, I have asked two questions of every text. Firstly, what tacit or explicit judgements about the nature of science are being made? Secondly, how might these judgements impact on the way the writer portrays the art-science interface? This method of selecting and judging my reading makes another point clear. In all that follows, I am especially interested in developing examples of types of discourse about the science-art interface, pointing out their relation to various epistemological traditions.

In this review therefore I group my texts into sections, corresponding to a number of themes. Section II considers those texts which, though not formally academic, buttress their responses to contemporary art-science interaction by aligning themselves with ideas about the process of science, such as its methodology or its putative autonomy within the wider culture. Sometimes that alignment is a clearly focussed commitment to a particular philosopher, for example Karl Popper (Arends and Thackera 2003). Other times the assumptions are less formally articulated, but can nevertheless be excavated from the way an author asserts the differences, or similarities, between art and science (Snow C.P.1959, Hafner 1969;; Byatt 2000; Campbell 2000; Ede S 2000; Cohen 2000, 2002).

In Section III I draw on a larger and more academic class of texts that includes material generated by writers in the history of art and of science, and in literary and cultural studies. We shall find that, at least if they are writing in the last 20 years, these authors follow convention in shifting away from the received view (Suppe 1977) and from positivism. They are inclined to find highly problematic the concept of influence in science and art, look to culture rather than formal epistemology for articulating the actual boundaries between art and science, yet are committed to searching for some way of describing the processes that put art and science into relation with each other (Jones and Galison 1998; Vargish and Mook 1999; Kemp 2000; Miller 2001; Henderson 2004). What these texts have in common is not so much a formal interest in epistemology as a desire to use concepts of cultural production and interchange for understanding conceptual traffic between art and science.

As I have suggested, another set of academic texts emerges from the philosophy and sociology of science. These are texts wishing to give theoretical and empirical strength to arguments that contemporary scientific research is irrevocably social, never purely technical.

To make a few distinctions here, scholars commonly see the Edinburgh school, or strong programme, as combining a commitment to the concept that the scientific knowledge is influenced by social factors(Bloor 1976) with the clear implication that the distinction between the 'technical' and the 'social' remains real and important. A shift is seen when we turn to another strand of work, namely laboratory studies, where, this distinction is seen as problematic or false (Latour and Woolgar 1986; Pickering 1995; Knorr-Cetina 1999).

Another theme explored by the science studies community is that of scientific practice. Such texts not only assert the value of sociological enquiry, including ethnographic research, as appropriate for understanding the causes of scientific knowledge; they also inaugurate reflexive debates over how social scientists should conceive their own practice, for example how they write about the drifting formations of scientific knowledge, practice and culture (Mulkay 1991, ch12; Knorr-Cetina 1999, pp265-298). My own emphasis will be on how philosophical and sociological ideas associated with laboratory studies or practice in science might be further developed by analysing relations between scientific and artistic practice.

Necessarily the development of my reading, and thus of this literature review, was affected by the things I felt I was learning from the interviews with scientists. Scientists invited to discuss their collaborations with artists often revealed their tacit assumptions about the nature of science. As discussed later, my interest in this issue developed to a greater and greater extent during my progression through the series of interviews, and I adjusted my questions accordingly.[40] There were many examples of a scientist being cautious about admitting any direct influence on his research, while prepared to signal very clearly the impact of artistic influence on the professional culture. This strategy of the scientists to allow shifts in their

[40] See chapter 4, section IV.

view of their culture, but not in their 'science', struck me as obviously problematic. From the vantage of SSK, the device is clearly unsustainable. This experience from the interviews encouraged me to scrutinise texts for signs of such tensions (Ede 2000), noting scepticism for the view that art has a role in the scientific enterprise (Wolpert 2002), while contrasting it with sympathy for that position (Kemp 2000).

Inevitably in a review of such a heterogeneous field, I have to make some brief excursions away from questions about art-science demarcation. For example, might the important point about my chosen collaborations be not so much the way they exhibit a scientist working with an artist, as that the scientist is simply working with someone else? Could very much the same set of principles and issues been obtained had these scientists been collaborating with sportspeople, or economists, or shop keepers? The act of collaboration with an artist might be fraught, or fruitful, for a number of reasons; but perhaps the same could be said of any collaboration, including a science-science collaboration. The small tensions and ambivalences that emerge in the interviews seem to involve both normal scientific life, as well as the collaborative life with artistic practice. Although this study will argue that the nature of artistic practice brings certain consequences for scientific research, there may be important aspects of the art-science collaboration which would be common to all collaborations. These aspects, we shall see, seem hard or impossible to disentangle. In fact the concept of ambivalence – centrally important to this study - has been recently explored in relation to collaboration between scientists. I end this literature review, then, by looking at a number of sociological studies of collaboration among scientists generally (Laudel 2001; Hackett 2005).

II. Contemporary texts and themes

In a Preface to *Strange and Charmed*, a selection of essays about the contemporary art-science interface (Ede 2000), the novelist A.S. Byatt wrote, "The artistic 'culture'... differentiates itself from the scientific culture by cherishing the individual gesture and scribble, and very often by characterising itself as the subversive, the destabilising, the contrary". If Byatt is blunt in her assertions about the differences between science and art, she is equally clear in linking those differences to current maladies in intellectual culture. For part of Byatt's interest in a differentiation between the arts and the sciences is prompted by certain deplorable developments in the humanities. She sees the humanities as having entered "an age of suspicion", where the ideas of objectivity and reason are considered illusory. A consequence of this trend is the growth of the belief that "our language and our circumstance speak us, not we them". As a result the humanities are committed now to "...solipsism, to navel-gazing, to a sense that the inside of our head [is] all we could know..." (Byatt 2000, p7).

The terseness of a book's Preface perhaps excuses Byatt's swift sketching. The interesting point is that she sees science as the corrective for a culture now lost in self-absorption, for "...[this] complacent mental misery makes no sense in the world of scientific discovery". Byatt concludes: "Curiosity doesn't kill cats, it saves them. It is a fundamental human drive, and the opposite of solipsism".

Here we have our first example of a belief about science being used to both show up, and then cure, a sickness in the artistic project. It is not simply that contemporary art has lost itself to narcissism. Byatt's point is that science is objective. Its beliefs are representations of nature, not of the scientific mind. What is more, these methods and beliefs can transfer to, and transform, the artist in danger of ending her days in an abyss of introspection.

Byatt's position is bracingly clear. In the light of recent philosophy of science, it is also highly problematic. The point worth debating is not so much the validity or usefulness of Byatt's description of the disabling affects of "solipsism and navel-gazing" (we are not given examples). What we must note is the assumption that science gets its value from an alleged freedom from the vices of post-modernism. These vices include of course a principled suspicion of authority and expertise, the very qualities that Byatt imagines are unequivocally present in scientific belief. To put it another way, artists can effect a rescue from their maddening narcissism by collaborating with scientists, or at least by boning up on their textbooks. Science is not only positivistic; it is optimistic too.

Science, then, will influence art, and apparently in important ways. How in fact does Byatt suggest this influence might act? A brief Preface cannot give details, of course. The details she does give, however, talk of the hardware of science. She mentions the fascination an artist might have with "cameras and optic fibres, radioactive isotopes and x-ray films, microscopes, telescopes and cellular materials" and describes as "splendid" the way some artists are working with flesh and blood, embryos and sperm (Byatt 2000, p9). There is a small irony here. Where one might have expected references to the glories of mid 20th century epistemology (with its emphasis on order and theory), the hint instead is to a jumbled-up science that is machinic, craft-based and practical. Byatt's interest in technique unintentionally evokes the practice-centred account that was initiated by Ian Hacking (1983), later to be adopted as an important plank of social constructivism (Pickering 1992).[41]

Byatt sees in language another example of scientific superiority. Her exemplar however is not drawn from the tradition in science that sees epistemic authority resident in language that is austere and cautious - the habit of mind we owe originally to Sir Francis Bacon and the founders of the Royal Society: "... a poor and unskilful code of words incredibly obstructs the understanding" (as cited by Jardine and Silverthorne 2000, p42). Byatt alights instead on something surely much rarer in contemporary scientific discourse, namely the language of aesthetics. Scientists, Byatt claims, use language now vanished from the studio, but whose return would be welcome: "Scientists, without embarrassment, use the words 'beautiful' and 'elegant'..." (Byatt 2000, p10).

My concluding point is that Byatt is setting up a familiar polarisation between the potent and objective workings of science, and the febrile, narcissistic, and essentially lost meanderings of the artistic enterprise. Yet it is crucial to note that Byatt is reluctant to deny completely a donor role for art in any art-science engagement. Artists, it transpires, do bring valuable material to the table. As Byatt puts it: "[artists] ...often bring an alien sense of order and connection to a scientific object or concept, that can reveal new things in it". There is no detail here but given the earlier description of the weakness of the artistic intellect, it is striking to hear that artists can nevertheless "reveal new things" in science (Byatt 2000, p10).

[41] 'Social constructivism' is a broad term used by sociologists of scientific knowledge to capture the idea that scientific knowledge has social causes.

To summarise this part of my discussion, it should be apparent that Byatt's argument contains two oscillations in its position. On the one hand science is described in the abstract as objective, rigorous, realist; qualities we associated with the received view (Suppe 1977). On the other hand Byatt admits the significance of its machinic, practical content, a stand that reminds us less of classic philosophy of science (with its emphasis on theory) and more of its contemporary interest in practice (Hacking 1983; Pickering 1995; Galison 2000). We see Byatt, in her description of the art-science interface, inadvertently mixing philosophies of science that in specialist argument would be entirely distinct.

The second shift, or oscillation, concerns the role of art in these encounters. On the one hand art is decried as lost in introspection and fantasy. On the other hand, though its post-modern development has been distorted by "recondite theorists – Foucault, Derrida -" (Byatt 2000, p7) art retains nevertheless some purchase on science. Art has an ability to work at a level of analysis ignored or unperceived by science, but which might nevertheless be useful. Thus when Byatt claims that art can bring an "...alien sense of order and connection", there is an immediate reminder of texts that commend to science the importance of theories of emergence, complexity and chaos (Stewart 1998), and that argue for the reality of ontological and explanatory levels that operate above those normally occupied by natural scientists (Dupré 1993). Perhaps the most explicit description of how such a link might work between art and science is Kemp (2000, pp177-181).

We can take this description of Byatt's stand as pointing to a possible research programme. Byatt's image of science stands out of kilter with modern philosophical and sociological scholarship. Rather than simply rejecting this image as incorrect, a more suggestive and useful stance for the current study might be to see in Byatt's words not so much an epistemological account, as the exploration of a symbol. For Byatt, science symbolises what the indulgent modern artist has allegedly cast aside, namely rigour and objectivity. Science is re-cast as the intellectual and moral template that forces structure once more into the floating world of the drifting contemporary artist. Scientists, it is assumed, are the willing guardians of the template. Our artists would do well to draw near.

We shall find in the analysis chapters that the matter is more complex. It seems possible to construe the scientists as themselves attracted to an artistic practice they symbolise as subjective, personal and expressive. There are constant echoes of this theme in the transcripts, with Byatt's symbolic accounts emerging – more to be challenged than supported – in the interviews. Here is an example, where the artist Jane Prophet discusses her scientific partner Neil Theise:

> JP When I first met him he was very keen to show me his images, because they were so 'beautiful', and he thought I would warmly and strongly relate to him as a scientist through his scientific images.

But :

> I wasn't very interested in how aesthetically beautiful they might be in Platonic ways, I was just much more interested in the role they performed for him.[42]

And here is the scientist Ian Thompson describing his work with the artist Paddy Hartley:

[42] See transcript, p228, line 50.

IT So our first exchange was him helping me make implants; so the two of us stood together by the furnace and made the moulds there and then, melted the glass, pour that into the moulds...[43]

I suggest that these accounts of an artist herself rejecting a scientist's use of 'beauty', and of the scientist benefiting from the sculptural skills of the sculptor Paddy Hartley, challenge the truth of Byatt's assertions about the nature of science (and of art). The first of my analysis chapters, Chapter 5, will suggest there are rather clear examples of artistic practice that impact on science, and will set out ideas that stand in symmetrical opposition with those of Byatt. I shall explore the possibility that the scientist, embarking on a collaboration, finds utility in the very qualities Byatt is warning us about. Often enough in these collaborations, actual artistic practice provides skills that fit well with the scientific project. It is not just the practical skills that scientists value for their seamless application to science. I will show they can also be interested in, and inclined to embrace the qualities of art that make Byatt so fearful. That is, as well as finding value in artist's rigour and material skill, they also are drawn to what they see as the vulnerability, imprecision and subjectivity of artistic practice. A theme to be explored is how a scientist might aim to 'import' these qualities into a matrix of scientific and professional codes.[44]

Sian Ede rehearses some of these points further in her own contributions to *Strange and Charmed* (Ede 2000). It is interesting, given the way in which issues to do with education and careers are often mentioned by my interviewees, that Ede decides that a good way to evoke the distinction between art and science is to write about the classroom. No specialist

[43] See transcript, p173, line 21.
[44] The research questions listed at the end of this chapter lay out the structure for my enquiry into these points.

knowledge of epistemology (or of education) is needed for seeing in the following quotes symbols under construction, rather than an accurate account of classroom life.

> ...although school science still has an old-fashioned feel to it, the school laboratory is a version of professional or academic laboratories. Potentially dangerous places where an attitude of seriousness and responsibility is required, they are domains where certain protocols are set down and a particular language is spoken.... the main currency of that communication is logical thinking, mathematics, the application of graphs and the writing up of work, usually in numbered paragraphs, all to serve one of sciences fundamental concepts, the role of evidence (Ede 2000, p31).

And:

> Science is serious, methodical and incremental (one learning step leads to another). Its basic method for the examination of any phenomenon is to narrow down the parameters for scrutiny and record data systematically, often just along two axes on a graph...*individual opinion is inadmissible* (Ede 2000, p32 emphasis added).

While:

> Art rooms, in complete contrast, are places where personal expression and individual creativities are fostered, where, unless the teacher is unusually dogmatic, there is no 'right' way of doing anything, and where a kind of glorious mess prevails. The science department and the art room are a universe apart (Ede 2000, p32).

In this and other descriptions Ede gives us an image of science where theories bring us knowledge of the world, while the arts community remains "preoccupied with its own belief systems". Further, that preoccupation entails an alienation from serious public life, whereas science occupies the intellectual high ground. It is striking to see the remarkably favourable territory Ede grants to science: in touch with public life, in touch with nature, in touch with the intellectual world. The remarkable phrase used to describe school science education ("individual opinion is inadmissable") serves to create an alliance between the arts world, and those who condemn recent attempts to enliven the school science syllabus (Perks 2006); more usefully it provokes the thought that an important aspect of the art-science dynamic will be its value in curriculum design.[45]

As with my discussion of Byatt, the significance for my study is not so much in the distance Ede puts between her own position, and that of post-Kuhnian philosophy of science, contentious though this is. It is the fact that these comments are made in the context of the art-science interface, and set up a symmetry between an uncomplicatedly objective science, and a queasily self-regarding art. Nor is this dichotomy likely to be undermined by those sociologists, who have relegated the scientific method to "...a league of the meretricious" (Ede 2000, p41). It seems significant that even when Ede briefly surveys the alleged cultural gulf between the academic humanities or social sciences, and the natural sciences, the same terminology emerges. For example Alan Sokal is described as possessing common sense, while Bruno Latour is allowed the more artistic talents of "sophistication, wit and complexity" (Ede 2000 p42).[46]

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[45] I selected one of my interviewees, Mark Miodownik, for his work in setting up a new course at Kings College London, 'EngineeringArt'.

[46] Alan Sokal instigated the 'Sokal Hoax' in 1996 by writing a joking article for *Social Text* that was taken too seriously by the editors. The controversy over the ruse vastly amplifed the so-called 'Science Wars', whose key

Ede also suggests that notwithstanding her diagnosis, some kind of rehabilitation for art is possible, even emerging. That recovery, she implies, is due to the kind offices of scientific thinking:

> At last, then, artistic expression seems to be moving away from the existential angst and self-defeating ironies born of the political and psychoanalytical preoccupations of the recent past, towards a tougher, scientifically and technologically-informed curiosity, both clinical and passionate at the same time (Ede 2000 p22).

It is tempting to construe Ede's view of art's relation to science as essentially that of the neurotic patient about to be steered back towards a normal life by the attentions of a robustly objective therapist. But is there no place for art within the scientific process? Like Byatt, Ede signals that there may after all be some active role:

> ...in turning to science, artists force *a human perspective into those chilly disorientating environments*. If scientists try to abnegate the human element in order to observe nature with the utmost objectivity, artists, in contrast, place human concerns and imaginative perspectives at the centre of their work. Facts are interesting, but surely only ever in relation to ourselves and how we live our lives? (Ede 2000 p22 emphasis added).

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text *Higher Superstition* (Gross and Levitt 1994) included a strong attack on the science ethnomethodologist Bruno Latour.

Further, in an account of the possible market for her book *Strange and Charmed*, Ede suggests:

> It [the book] should also interest scientists and some of the wider public who may gain a perception of how, *conversely*, a knowledge and understanding of contemporary art can *influence the practice* of science" (Ede 2000, p11 emphasis added).

It turns out that these ideas, only loosely drawn out by Byatt and Ede, are encountered in much more detailed form in the analysis of the transcripts that makes up chapters 5 and 6 of the current study. In the emphasised first sentence in the excerpt above, the suggestion is made that artists, in effecting their own rescue through contact with the sciences, find themselves not only a beneficiary, but also a potential asset, able to bring a human dimension to cold, "disorientating" environment. How the artist's human concerns and imaginative perspectives might differ from those of the scientist, and how the artistic version might impact on scientific knowledge, is a problem left undeveloped. Note however that art's ability to enter into the scientific life depends on a prior orientation with the scientific method[47], perhaps a reasonable position were it not for the way representations of science have become so contested.

We have seen that Ede likes to twin as separate poles the baleful state of contemporary art with an idealised, adventurous, successful science. This symbolising however is not allowed to close down entirely the possibility that artists may have some impact on the scientists. It is

[47] For Ede the orientation occurs through taking on a "...tougher, scientifically and technologically-informed curiosity..." (Ede 2000, p22). The point resonates strongly with Knorr-Cetina's characterisation of the lab as something that re-configures all comers (Knorr-Cetina 1999). There seem few other points of agreement however between the two writers.

intriguing that this impact depends both on the artists' humanity, and their scientisation. They must retain their humanism while avoiding excessive introspection, no doubt a difficult balance. Dealing with this 'mix' or 'oscillation' is one of the tasks of the analysis chapters. For the purposes of this literature review, it is worth simply drawing out a paradox. In somewhat outspoken and brusque fashion Ede draws the starkest of divisions between the artists and the scientists, and reserves her scorn for the former. Yet she implies that, however severely artists have been derailed by postmodernism, properly re-trained they nevertheless have the ability to bring something to the work being done by scientists.

Ede and Byatt come from the humanities. Can we find scientists making the same point?

The following account shows these issues under further discussion by the scientist Lewis Wolpert and the artist Andrew Carnie. The exchange took the form of a review in *The Observer* newspaper by Wolpert discussing the Wellcome Trust-funded exhibition 'Head On: Art With the Brain in Mind' (Albano et al 2002) at London's Science Museum, with a reply a week later by Carnie.

Wolpert wrote:

> The current vogue for believing that art and science should be brought together and share much is strongly promoted by the Wellcome Trust that gives many thousands of pounds to art/science projects. Moreover, in the pack that goes with the Trust's new Science Museum exhibition bringing artists and neuroscientists together, Dr Raj Persaud says that understanding the universe might also need art, Baroness Greenfield that the two are merging, and James Lovelock expresses the belief that they have much in common. I completely disagree (Wolpert 2002).

He concludes: "Although science has had a strong influence on certain artists – in the efforts to imitate nature and thus to develop perspective or in the area of new technologies – art has contributed virtually nothing to science".

As with the comments of AS Byatt and Sian Ede, these comments are richly suggestive for the current study. In my discussion of Byatt and Ede, I emphasized the startling distinctness of the qualities these writers attributed to contemporary art and science. Wolpert, it should be stated, is writing in a newspaper and must make his argument bright. That much admitted, his attributions about art and science are as clear, and as philosophically contentious, as our samples discussed earlier.

Two attributes are worth emphasizing. The first concerns the role Wolpert assigns to scientific explanation:

> Science is about understanding how the world works, there being only one correct explanation for any observed phenomenon (Wolpert 2002).

It is hard to imagine a statement more challenging to current thinking in science studies. Though there is no elaboration provided, it is tempting to interpret the words as an effective slogan for the merits of a genre of beliefs – realism, monism, the unity of science, reductionism – that while still important, are highly problematic if used as descriptions of science. That genre of beliefs has been called 'the received view' (Suppe 1977) and in the light of these comments by Byatt, Ede and Wolpert it is worth making a brief diversion to explore it more fully.

It is an important aspect of the received view that the barrier between science and other fields of culture is formidable. It is a barrier built from a network of methodological proposals. An example of such a proposal is the existence of a distinction between the language used for dealing with observations, and the language used for dealing with theory. An observation is taken to be a 'reading' of a natural phenomenon, articulated in a scientific language that allows judgement of scientific theories as true or false. The separability of the observation language from the theory language allows a representation of the former as in principle objective (uncontaminated by personal or social values), and is an important bedrock for realism – the idea that the success of science is due to it being able to represent nature faithfully. The demise of the received view was in large measure due to the insight that the separation between an observation language and a theory language is bogus. Theory is present in all observation, and shapes it (Hanson 1969; Suppe 1977, Hesse 1980).

By Kitcher's account (Kitcher 1993), the received view (he calls it 'Legend') is a myth that celebrates science and scientists, and considers its task the attainment of truth. 'Legend's' claims vary. Some are ambitious, and see science as discovering the whole truth; others are more modest and concede that science goes after truths that are attainable, or important to us. Either way science makes progress and gets closer to the truth. Central to the task is discovery (the reading of nature), which is itself controlled by the methodological canon. The canon has been much articulated in the 20th century by philosophers of science, but a version of it forms part of the intellectual make up of the trained scientist, and is therefore evident in the transcripts of this study's interviews. We associate this canon with the making of hypotheses, their elaboration, the careful design of apparatus, and the disciplined testing of the hypothesis by observation. Logic, discipline and an unwavering faith in the possibility - indeed the necessity - of objectivity, are all central. All these matters help explain the influence of science on society. It is the 'royal road to truth'. They also serve to support the related argument that science is the most important form of scientific knowledge, and to warn against profane attempts to make comparisons with other forms of culture, or worse, to pollute it with 'outside influences'[48]

Kitcher sees this view as part of the "dear dead days" and locates its demise as dating from the late 50s. He suggests that scientists have an ambivalent attitude towards 'Legend', sometimes upholding it, sometimes undermining it, a possibility that the interviews of this thesis will explore.

Wolpert, it must be admitted, is not ambivalent. One of his points is that "art is not constrained by reality". Unlike science, he suggests, art cannot be shown to be wrong. Poking fun at the recently opened Tate Modern, he asks how he can know whether the decorators' trestle, paint and brushes standing at one corner of the gallery are a work of art, or simply tools left lying around. He suggests that in science the peer review system is a quality control method that art simply does not have, and is therefore without any systematic method of judgement. No particular expertise is needed for judging a painting, he suggests. Judgement, perhaps, is besides the point: "In what sense can a painting be right or wrong?" Wolpert asks. Unlike science, art is capable of many interpretations.

A second key tool of demarcation is the issue of the role of the individual. For Wolpert, the arts reek with individuality, while in the sciences the individual is superfluous: "no

[48] Isiah Berlin's writings on the Enlightenment invite the reflection that the received view would allow influence between science and the humanities, but in one direction only – from the former to the latter, and this by virtue of the sciences' (assumed) epistemic superiority. For an imperious description of the moral background to the received view, it is worth quoting Berlin more fully: "I begin with a tradition in which many eminent scientists today still stand: the tradition of those who believe that it is possible to make steady progress in the entire sphere of human knowledge; that methods and goals are, or should be, ultimately identical throughout this sphere; that the path to progress has been, as often as not – or perhaps a good deal more often – blocked by ignorance, fantasy, prejudice, superstition and other forms of unreason; that we have in our day reached a stage when the achievements of the natural sciences are such that it is possible to derive a single integrated set of clear principles or rules which, if correctly applied, make possible indefinite further progress in the unraveling of the mysteries of nature" (Berlin 1997 p80).

Shakespeare – no Hamlet; no Picasso – no Guernica". In other words: "Unlike the arts [science] is a collective endeavour in which the individual is ultimately irrelevant – geniuses merely speed up discovery."[49]

Wolpert is not delayed by considerations of how accurate these representations are. For example, a more nuanced approach might also wish to see distinctiveness, but explore the fact that art has schools and fashions (analogous to peer review), and traditions that set constraints (which limit individuality); while the history of science can be construed as a succession of theories, their succession not necessarily best described as a royal road to truth, and thus not so easily agreed to be the development of a set of single, correct, explanations.[50] Wolpert's simple demarcation depends on a decisive rejection of views of science emerging from philosophers, historians and sociologists and thus provokes the contrasting idea, to be developed throughout the thesis, that an investigation into possible conceptual and practical traffic between the sciences and the arts will find in science studies a useful resource for understanding such co-development.

Andrew Carnie's own response to Wolpert is to dispute that artists will necessarily remain outside the science research process. But once inside, what will they do? Far from claiming that in his work with the neurologist Richard Wingate, artistic practice began to infiltrate the Kings College laboratories, Carnie admits that their product was a work of art, in this case a photography-based installation. No mention is made of any impact Carnie might have had on Wingate. Nevertheless Carnie sees a role for artists.

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[49] For a refutation of this argument see 'The Parallel Fallacy: On Comparing Art and Science' (Topper 1990).
[50] For the development of the post-empiricist doubts that influenced Kuhn (1962), and undermine Wolpert's position see Quine 1951 and Hanson 1958. Suppe 1977 gives a comprehensive account of the demise of the logical positivists. The significance of the huge literature in post-Kuhnian sociology of science is covered in Knorr-Cetina and Mulkay 1983, Rouse 1987, Pickering 1992, Shapin 1995 and Fuller 2000.

> In the longer term, a collaboration like ours promises more thorough artistic participation in the scientific vision. As they gain maturity, we might well see artists spending more time in scientific laboratories, gaining formal scientific training, and maybe even becoming part of research teams, integrated into the scientific research itself (Carnie 2002).

And:

> The practice of science is not isolated from the world we live in. Science has its politics, its economics, its fashions even. And anecdotally at least, some of the scientists involved in "Sci-Art" collaborations report an intellectual engagement in them that goes further than personal enjoyment. It's not just that it allows them to get out of their labs a bit more; it also seems to offer a different way of looking at their own work as scientists (Carnie 2002).

Granted that newspaper articles are not good places to mount an extended argument, it is nevertheless interesting to see where Carnie has put his emphasis. On one hand, Carnie aligns himself with conventional sociological theory and rejects a vision of science as isolated. That porosity is important for the plausibility of his second suggestion, which is that artists might in time be entering labs and working there, and that their presence will bring more to the scientists than simply personal enjoyment. Carnie suggests we guard against the argument that science is isolated. However, the questions multiply. What might be this "different way of looking"? How might artists insert themselves into the scientific process? And if they did "become integrated into scientific research itself", perhaps even acquire some training, would they remain artists?

To start work on the detail of these questions, let us consider two texts published by the Wellcome Trust, both highly pertinent because they contain accounts by scientists who have worked in collaboration with artists (Arends and Thackera 2003; Arends and Slater 2004). These texts are accounts of projects supported by the Trust, accompanied by some editorial commentary. Here we do find some details on whether, and how, these science-art collaborations might impact on research. For example, Tony Holder, a parasitologist working at the Medical Research Councils National Institute for Medical Research and a recipient of Wellcome Sci-Art funding, asks: "As a scientist, how did I benefit from collaborating with an artist?".

Here is part of his answer:

> Perhaps making connections between different realities causes us to question our assumptions, examine with fresh eyes what we take for granted and encourage us to see things more inventively. Perhaps by stopping and paying attention, new ways of looking at the process will emerge and different perspectives result. At the end of the project I have a deeper, more holistic view of this complex subject (Holder 2003, p127)

In this case we see that though the question of benefit to the scientist has been allowed, the process of that benefit has been black-boxed into the ill-defined but familiar phrases "new ways of looking" and "different perspectives". However, the editors to the volume see the question as worth pursuing: "The intermingling of practices from the arts and sciences, a taste of which is presented in this volume, has prompted much questioning. What is it about science that attracts artists? Is it the richness of its subject matter, the moral vibrancy of the issues it raises, or the compelling strength of its methodology? On the other side, what does science gain from these encounters" (Arends and Thackera 2003, p7).

If the book's Preface asks the question, the book's Introduction risks an answer. The site of influence, the place where an artist's wanderings might indeed impact on the scientist, occurs early in the scientific process. Arends places the artistic influence at the point where scientific thought is described as loose and ill-formed, not yet tram-lined into the algorithms of the scientific method:

> This early and ill-described stage entails a 'methodology gap'. It is at this messy and amorphous juncture, where the creation of knowledge starts, that scientists find their inspiration. It is an opening that offers opportunities to collaborations between art and science" (Arends and Thackera 2003, p10).

Parallels are being drawn with the philosophical distinction between the context of hypothesis formation and the context of justification, (the province of the scientific method, and the territory of philosophers). It is in the first stage, where the first ideas are conjured up, even before pressed into the format of a testable hypothesis, that art appears to have its role. The way this role is enacted, however, remains mysterious.

> It would be fatuous to think that art is like science or vice versa, or that the combination of the two could create a new academic discipline. Equally, it would be short-sighted to assume that the two disciplines cannot engage in a meaningful and productive debate. The research that follows offers ample evidence that *mutual*

> *inspiration* can generate new transdisciplinary knowledge and that *fresh perspectives* have the potential to invigorate creativity in both domains (Arends and Thackera, p14 emphasis added).

In terms of research questions, it seems clear that if scientists are our object of enquiry we so far have acquired a theoretical background that does not go much beyond claims of 'mutual inspiration' and 'fresh perspectives'. In the quotation above, the use of the words 'generate' and 'invigorate' arguably evokes the Popperian sense of creative forces (they include art) finding their place in the inchoate massing of ideas that is the start of the scientific process.

However, in such accounts, and in this study's analysis chapters to come, there is a well-explored element so far unmentioned in this literature review: the scientists' engagement with the public. In these two Wellcome texts, and in my interview transcripts, there is constant reference to the scientist finding an audience; the artist, it is suggested, acts both as the scientist's interlocutor, and the mediator between science and public[51]. Some, but not all, of the scientists I interviewed are known too for their work in public engagement generally.

Here we have another oscillation, another uncertainty. The art-science collaboration, it appears, might have a number of roles. It may be styled as an example of public engagement with science; it may be a research project, and it may be other things beside. An experienced practitioner, Jane Prophet confirmed this plurality, and pointed out that an individual collaboration might shift from one form to another: "There are lots of different sorts of

[51] Approximately 70% of the public engagement projects funded by the Wellcome Trust have an arts component (Hurren 2006.).

collaborations. You often start off thinking you are doing one, and then it becomes something different".[52]

The Wellcome Trust's leaflet promoting the 2006 call for proposals captures well the shifting forms a collaboration might excusably take. The grant call was designed "...to support and encourage imaginative and experimental arts projects that investigate biomedical science". Successful applications "will ...provide fresh and exciting ways of interacting with scientific research – intriguing and captivating spectators and engaging a wide range of audiences in scientific issues. In turn, science – with its complex contemporary advances and the social ethical and cultural implications – offers a rich supply of inspiration for the arts..."

Earlier, Ken Arnold, then the Wellcome Trust's Head of Exhibitions, had emphasised the benefit for public engagement: "...the true power of science and art as a union is not what science can learn from art in terms of practical breakthroughs, but what it can gain from art in terms of building a more engaged relationship with the public" (Arnold 2002, p3). Yet the Trust's views were not necessarily prescriptive. Now working as Head of Public Programmes at the Trust, Ken Arnold told a BBC Radio Four audience in 2007 that "We started pretty defensive about all this [the role and value of 'sciart'] and have become somewhat more relaxed and open to being less goal-directed" (Arnold 2007).

Putting these messages together, the main implication is this. The stated position of the Wellcome Trust appears to be that artists prise open and re-cast scientific findings, and prepare them for a wider audience. Art is a mediator, a translator. But we have also seen intimations in the literature discussed so far, some of it from the Wellcome Trust, that art can influence science itself, and not only its public face. In sum, from the literature examined so

[52] See transcript p231, line 159.

far, there seem three possible research themes that might plausibly illuminate a study of the current art-science interface. Very baldly stated, they are:

- Art might benefit through gaining ideas, techniques.
- Science might benefit because of artistic assistance in thinking and doing.
- Science might benefit because of an improved relation with the public.

The view from *Nature*

It is interesting then that Philip Campbell, editor of Nature Magazine, and something of a patron in the area, disputes two of the points of this triangle, when he questions "...the appropriateness of an implicit goal that such collaboration will promote the public appreciation of science. That smacks of a de-inspiring political correctness" (Campbell 2000 p5). If Campbell wouldn't want 'sciart' to be solely a vehicle for cajoling the public he also joins Wolpert in doubting the impact of art on science. He observes that artists and scientists make "messy bedfellows". When collaborating their interests are sometimes "at odds". It is "...obvious ... that their objectives, cultures and sociologies are quite different". Campbell's assertions seem in fact to be heavily influenced by the two Wellcome Trust reports on the early progress of the sciart awards scheme (Cohen 2000, 2002). Claire Cohen, a management consultant and academic at Kingston University, UK, had written on her own initiative a brief study of the sciart scheme in 2000. The Trust then invited her to follow this up, in 2002, with a second report[53]. According to Campbell, the first study includes the conclusion that artists tend to get more out of these projects – and often put more into them – than scientists, a notion my own interviews might cast doubt on. He notes that "...as Ms Cohen also reports, [the scientists] see minimal influence on the science that ends up in the published literature".

[53] See also the Prologue to this study, p1.

Campbell dwells considerably on these issues. He asserts that scientists can gain personally from art-science projects, but that art and artists will be the main beneficiary. Returning to the alleged coyness of the scientists, Campbell guesses that "...if the recalcitrant researchers had found their collaborations scientifically stimulating time would have been found".

Campbell's views repeat some of the ideas I discussed earlier in relation to Wolpert and Carnie[54], but there are fresh questions here that need examining. In Campbell there is a cultural awareness; in a simple way, issues of the art-science interface are mapped onto a view of science that takes seriously the actual practice of scientists. In the few sentences quoted above, we can note a distinction between science-as-culture, unfolding daily in the labs, and the science that ends up in the published literature. This is clearly an important distinction for an investigation into art-science interchange: if art is having an impact, is it acting at the point of scientific practice, or is it there too in the published record?

Note the point that scientists are recalcitrant. Most starkly, Campbell repeats the point that has now become familiar: whatever scientists gain personally, artists will be the main beneficiaries. In Campbell's sphere of reference, a 'personal' gain is distant from any implication for science. This was a theme that also emerged often in the interviews. Interviewees would frequently discuss personal aspects of their professional life, such as the way their interactions with colleagues were affected by their art-science collaboration (this was a theme discussed by Mark Lythgoe). Less often, interviewees explored how and whether these activities impacted on laboratory practice. Though rare, such explorations do

[54] See pp60-64.

occur from time to time in my interviews (for example with Philip Kilner and Ian Thompson) and are richly suggestive.

It is worth noting that the journal Campbell edits, *Nature*, has played a significant part in commissioning comment on the contemporary art-science interface. From 1997 the art historian Martin Kemp has written a regular column, variously titled 'Art and Science', 'Science and Image' and, most recently 'Science and Culture'. Many of the articles were in time collected in book form (Kemp 2000).

Nature's commitment to the topic was further signalled in 2005 when it published a complete supplement *Artists on Science: Scientists on Art*[55]. Contributors included novelists, poets, painters and scientists. Ian McEwan and AS Byatt discussed the role of science in their work. Patrick Cavanagh of the Vision Sciences Laboratory at Harvard University examined various works of art to show what they tell us about the brain: for example explaining why inconsistent lighting is not much noticed, how optical illusions work, and how the brain can connect up disparate shapes and lines to construct and image (Cavanagh 2005).

In the supplement too the film maker and teacher Ken McMullen discussed his setting up for 23 artists a residency in the Geneva particle laboratory CERN. McMullen's description of the work includes a theme already highlighted by my study, that is, the gains artists can expect from getting to grips with the real world: the artists' final show, he writes, "...was motivated by the belief that artistic practice, if it is to have meaning in the modern world, cannot derive from the artist's subjectivity alone, but demands engagement between the artists [diverse] processes and the external world in which we live". (McMullen 2005). But,

[55] Nature 434, 294-323 (March 17, 2005).

as for the experience of the CERN scientists, McMullen is silent; and in fact the theme of scientists' gains in working with artists remains unexplored in the *Nature* supplement.

III. Themes in the Academic Literature

Turning now to a more academic literature, one of the most famous accounts of the art-science interface is C.P.Snow's 1959 Rede Lecture in Cambridge. CP Snow's anxieties have been criticised as somewhat shallow[56]. It is unreasonable, as Snow was himself later to admit, to believe that intellectual culture could simply be bisected. And though his lecture may have made the phrase *The Two Cultures* an entity of remarkable resilience and resonance, Snow's points must be seen against the backdrop of Snow's Cold War fears of the Soviet Union, and a view that poverty in the developing world will be solved by science and technology[57]. My discussion of Snow, here, is mostly concerned with what is a side-issue in *The Two Cultures*, namely the cultural isolation of scientists and its possible implications for their intellectual life.

Snow's lecture is remembered largely for its diagnosis of a malady in intellectual life. The community of scientists he worked with, and the literary scholars he also worked among, neither communicated with each other, nor wanted to. In going from "...Burlington House or Sth Kensington to Chelsea, one might have crossed an ocean" (Snow 1998, p2). It is the allegation of dysfunctional communication that has endured. A closer reading, which quickly

[56] Leavis' sour reply is partly a complaint that a novelist who Leavis rated poorly should dare to criticize the literary scholars (Leavis 1962).
[57] Collini's excellent Introduction to (Snow 1998) reminds us that the lecture was concerned less with problems of communication between different types of scholar, and more with the global ambitions of science (Collini 1998).

suggests that Snow might have had in mind literary critics as his representatives of the humanities, also shows that the main losers in this failed marriage live far away in the developing world. For our rulers, like our literary critics, do not appreciate the powers of science to improve the lot of the poor, and fail to harness the powers of science in anything like the manner needed.

However, looking at the lecture from our own point of view, what are Snow's concerns about scientists? First we note that Snow's position seems to contain the mark of animosity: "If the scientists have the future in their bones, then the traditional culture responds by wishing the future did not exist" (Snow 1998, p11). But, for Snow, is the scientist missing anything? Does the fact he has the future in his bones mitigate sufficiently against the absence of discussion with literary folk? In his backing for their broad-mindedness, Snow points to scientists' interest in long-playing records and colour-photography (Snow 1998 p13), but then admits that when it comes to books (he means novels, histories, plays and poetry) the scientists are disadvantaged.

One of Snow's main concerns, it turns out, is the consequence of specialisation, and the isolation of scientists from the outer world of moral debate and political decision-making he himself inhabited. Both these views are echoed strongly in my interviews. Snow, reflecting later on the wide interest in his lecture, traced its impact not to originality, but by contrast to the fact that "a nerve had been touched" (Snow 1998, p54).

It is the particular form of Snow's anxieties that takes our attention, and offers focus. For these are anxieties that contemporary scientists share. In interview, the scientists discussed their role in culture; it was a concern of Snow. But what were Snow's views of the sensibilities of scientists? We know that Snow considered the political power of the traditional culture a negative force, likely to constrain science in its abilities to solve world hunger. But did he think that traditional (literary) culture could ever make any contribution to the scientific life, other than in recognising its proper importance and affording it political support?

Only once does Snow address the matter, but then in a restricted sense that relates to morality rather than epistemology. It isn't that scientists don't have interests in the moral or psychological sphere – they do, perhaps more even than those literary academics he was holding up to scrutiny (Byatt's accusation of artists' "solipsism and navel-gazing" seems to echo Snows distaste for the art world's moral evasiveness). The problem is that the:

> ...whole literature of the traditional culture doesn't seem to them relevant to those interests. They are of course dead wrong. As a result their imaginative understanding is less than it could be. They are self-impoverished (Snow 1998, p14).

That is to say, the amateurish but nevertheless deeply-felt moral and political beliefs of the scientific cadre could find no succour in the obscure, and backward-looking literature of the day. Where is the global vision, the optimism, in DH Lawrence, TS Eliot, YB Yeats, Ezra Pound and George Orwell? The list makes clear that scientists could find no common cause with the artists of the day. Scientists, then, do have moral, political and aesthetic sensibilities. Unfortunately the ability of the very people whose job it is to engage with those sensibilities and develop them, namely our artists, are unable to do so, according to Snow, because of their opacity and their pessimism.

It is obvious to Snow, in a way that would be more controversial now, that it is possible to make a clear distinction between a scientist's professional life, and his life as a morally-aware and politically-active citizen. If scientists are harmed by their isolation, the harm is to their imagination. Snow is silent on whether this admitted harm could have any affect whatsoever on the scientist's work. For our part we can imagine the tacit philosophy of science here – that science is latent in its method, and scientific method simply is a combination of objective measurement, logic, and hypothesis. In all that, there is neither need for, nor room for, anything that writers, artists, philosophers might say or do. The scientists might be impoverished, but the science itself will continue – so long as it is not impeded by the humanities-trained politicians.[58]

The view from Thomas Kuhn

Just three years after Snow's lecture, in 1962, Thomas Kuhn published *The Structure of Scientific Revolutions.* There are some remarkable parallels in the way both authors noted the huge interest in their work, and voiced regrets over a certain looseness of terminology. They each revisited their original text, and they each launched terms, "the two cultures" and "paradigm shift", whose validity is doubtful but which have a massive currency to this day. Kuhn was not a fan of the new sociology of science, and regretted the way his book was interpreted.

An established truth about *The Structure of Scientific Revolutions* is that its meaning has proved remarkably plastic to the mindset of the reader. Its significance as the pivot that

[58] An odd contemporary echo of this is the common complaint that our politics and media are dominated by 'humanities graduates' who allegedly do not understand science and are prone to doubt its authority. See Goldacre (2003, 2005)

allowed new emphases in the philosophy of science is never disputed, but the precise direction of the text remains elusive. Some have viewed the book as overturning views of science predicated on realism (notably, Rorty 1979). Read as an account of a switchback oscillation between different, even incommensurable, belief systems the text conjures a science free of progress, or a commitment to realism, and is grounded in methodological processes by no means unique to science. On this reading, Kuhn invites a dismantling of the philosophical apparatus that kept scientific knowledge separated from the scrutiny of the humanities and, especially, the social sciences.

Kuhn regretted such moves, and distanced himself from them. He was to put effort into clarifying his terminology, and into re-asserting the peculiar form of his socialized science. The social processes he drew up were the kind of relations to be found within the monastery, or the infirmary. To continue the metaphor, his interest was more in the way the monks organized their prayer and elaborated their doctrine, less in their precise relationship with God, or society outside.

While this internalist and conservative tendency of Kuhn has often been noted (Chubin and Restivo 1983; Fuller 2000), some interpretations also point out the enduring, living relevance of Kuhn to ethnomethodological work in science studies. Rouse has read *The Structure of Scientific Revolutions* as a call to downplay the significance of theory and observation in favour of a better understanding of science as a series of practices (Rouse 1987). A careful reading of Kuhn, Rouse suggests, evokes a science built of practical decisions. It is relations between theory and practice, their ability to function together, that grounds Rouse's own philosophy of science. Rouse's pragmatism I shall return to later in this thesis when I argue that his development of Kuhnian philosophy into a 'cultural studies of science', taken alongside a broad interest in the Continental hermeneutical tradition that seeks 'understanding' rather than 'explanation', might together prove fruitful for a properly dynamic account of the art-science interface.

On one occasion at least, Kuhn was to be drawn into such speculations, only to warn against the simple interpretation of his work as eroding the distinction between art and science. The occasion was a collection of papers in a 1969 edition of *Comparative Studies in Society and History,* a special issue devoted to cultural innovation, following a 3-day conference at Ann Arbor, Michigan, in May 1967.

In a contributed paper 'The New Reality in Art and Science', the Hampshire College art historian Edwin Hafner argued against any simple dichotomy between the contemporary arts and sciences. At first sight, however, his description of artists' work makes the divide seem unavoidable: "We are often told, and it is easy to believe, that the images of abstract art are not drawn from the real world. In the most conventional view of the modern school, abstract painting is a search for free expression of the artist's own vision" (Hafner 1969, p385). To make his point, Hafner collates a number of quotations from well-known contemporary artists and critics that describe visual art as best understood as non-representational, abstract and "in retreat from the world of sensible experience" (Hafner 1969, p387).

Hafner, however, argues that this so-called retreat cannot be a defining feature of art, for modern science itself had become abstract and non-figurative. In modern science, reality was a shifting set of concepts. One such shift, Hafner recalls, occurred when the real world of Aristotelian physics was not so much reinterpreted, as replaced by the Newtonian world, which itself in due course was overtaken by yet another world, relativity and quantum mechanics. Hafner's understanding of science is turned by two influences. Partly he was impressed by the implications of Einstein and particle physics. But, also, he was intrigued by Thomas Kuhn.

Hafner's dissolution of the art-science divide involves a number of arguments. He considers for example that both modern science and contemporary art are disturbing to a lay audience, because of their incomprehensibility, and because of their abstractness. Conversely, Hafner finds evidence that, notwithstanding their retreat from the recogniseable, art and science can produce artefacts that look very similar. He suggests that points of contact exist between art and science through the way aesthetic values are expressed in scientific images. Such images themselves present a challenge to the classic divide: "...it is an interesting and incontrovertible fact that the newest images of science and art are easily confused except by very special eyes". And he examines with sympathetic interest those questions that seek answers about the possible influence of the one upon the other.

Hafner advances his argument with some vigour. His project of challenging science's distinctiveness from art combines attacks on the opposing position with a portrayal of his own stand as beleagured and lonely. Speaking of an artist called to explain her work's similarity to some scientific visuals, Hafner describes her "passionate" denial of scientific influence as "extraordinary in several respects" (Hafner 1969, p394); and having praised Naum Gabo's "positive" view about the links between art and science,[59] Hafner suggests that Gabo speaks for a tiny minority opposed by most artists, critics, curators and academicians.

[59] "However dangerous it may be to make far-reaching analogies between art and science, we nevertheless cannot close our eyes to the fact that at those moments in the history of culture when the creative genius had to make a decision, the forms in which this genius manifested itself in art and science were analogous... Even for many theorists of art the fact remains unperceived that the same spiritual state propels artistic and scientific activity at the same time and in the same direction". Naum Gabo, quoted in Hafner 1969, p392)

There are fairly clear problems with Hafner's arguments, especially if they are applied to the situation to the years after 1980, when figurative traditions in art became once more fully evident. However we can see in Hafner two themes important for the current study. One is the way questions about distinctiveness travel with questions about influence. The other is the style of the discourse. As with writers mentioned in earlier sections of this literature review, so Hafner's style too is filled with anxiety and conviction. It was texts like this that alerted me to the way my interviewees coloured their comments on art and science with barely hidden animosities and scepticisms towards their professional culture.[60]

Though Hafner is demolishing the boundaries, there is only faint indication of how influence can run. Nor does Kuhn, whose work Hafner cites, give assistance. Still we note his sympathies. Having argued that art and science are truly inseparable, Hafner is at first sight more modest about how the two might influence each other. He writes: "While it can be asserted without question that art and science share the same conceptual material, it is far less certain that the work of one has exerted direct influence on the work of the other" (Hafner 1969, p391). He explores the idea by quoting from artists and critics who themselves consider the question interesting, for example Leo Steinberg and Naum Gabo (both arguing for direct influence) and Gyorgy Kepes and Robert Schenk (both arguing that the influence is not direct, but involves a shared interest in basic natural patterns, or is a result of living and working in a shared intellectual climate). In the following extract note how Hafner in the end falls back on metaphor and vague formulations to explain the commonalities between science and art:

[60] Of many examples in the transcripts, consider this quote from the interview with Mark Miodownik. Miodownik is discussing the attitudes of his colleagues: "It's very much – 'you can do this Mark, its your hobby, off you go but don't let it interfere with your real work'. This not being real work. And that frustrates me." See transcript p193, line 87.

> It is impossible today, just as it has always been in the history of our culture, for the artistic intellect to insulate itself from conceptual revolutions. When the winds of science shift to a new quarter, everything in their path bends a little; when we look at the resulting commotion, we see an image of the wind itself... An artist need no more understand mathematical physics than the waving grain understands meteorology (Hafner 1969, 396).

In sum, though Hafner is at pains to support the idea that currents of influence can run between art and science, he makes no attempt to describe how this could happen. Thomas Kuhn, responding to the paper (Kuhn 1977), is similarly cautious. In fact Kuhn is minded to foreclose the very project Hafner wants to support. Kuhn agrees with Hafner that his book *The Structure of Scientific Revolutions'* proposes mechanisms of scientific development that work well too in the history of art; indeed Kuhn's understanding of the history of art may have been central. Kuhn cites the work of Ernst Gombrich as an influence and as tending "... in many of the same directions". Talking of the result, *The Structure of Scientific Revolutions,* Kuhn agrees that one of its concerns is "... to deny, at least by strong implication, that art can readily be distinguished from science by application of the classic dichotomies between, for example, the world of value and the world of fact, the subjective and the objective, or the intuitive and the inductive." (Kuhn 1977, p340). This conclusion, for Kuhn, is unwelcome and disquieting. For one of his objects in writing his book had been to show that science can be demarcated from other fields, even if Karl Popper's programme of falsifiability is rejected.

It is important to note that Kuhn, who notes the influence of the humanities on his thinking about science, is quick to bear down on Hafner's tentative attempts to explore the possibility that science itself may learn from art. For example, in response to Hafner's exploratory comments that aesthetics may have a role in scientific development, Kuhn argues that aesthetics can never be decisive in scientific development. Aesthetics in science, writes Kuhn, are a means; in art they are an end. And while in art aesthetic decisions are put on public view, in science they remain private, "...painfully eliminated from their published work" (Kuhn 1977, p343).

On the one hand Kuhn can be interpreted as revealing structural similarities between the humanities and the sciences. On the other, *The Structure of Scientific Revolutions* is committed to showing that the differences are nevertheless clear, and that the processes of science are internal and autonomous. The implication of Kuhn's work must be that the basic stance of science is that of the defended citadel. Yet, accompanying the overt championing of its autonomy, there are ideas here about how science might relate to a wider culture. In particular it is worth noting Kuhn's comments on the role (or rather the lack of it) of the audience in the scientist's work, and his suggestion that empirical work will help us establish what, in fact, the differences and the similarities between art and science might be.

The view from two art historians

Part of my strategy in this literature review has been to compare two standpoints (Wolpert/Carnie; Hafner/Kuhn). Now, for an insight into contemporary scholarship in 'art-science studies' I shall pair, and contrast, certain aspects of the work of the Oxford historian Martin Kemp, with the work of the US-based scholar, Linda Dalrymple Henderson. The rationale is not so much that these writers adequately represent the field, as that certain differences I see between them usefully highlight some methodological issues to be explored further in the next chapter. I suggest that Martin Kemp brings to the debate a notable interest in metaphysics (Kemp 2006, pp157-158); by contrast, Henderson urges our attention be taken by the micro-processes of historical accident (Henderson 2004, p437).

Henderson's lengthy review of art-science interactions in the 20th century (Henderson 2004) discusses the question of influence, noting that the impact of "...art on science has always been the direction less discussed" (Henderson 2004, p433). Her attention is perhaps most intently focussed on early 20th century developments, for example cubism in art, the discovery of X-rays, the development of relativity theory and then quantum mechanics in science. Though Henderson's review makes no mention of contemporary art-science collaborations, her discussion of the concept of 'influence', especially her views on methodology for investigating traffic between art and science, has some important signposts for the present study.

The review appears as an introduction to a special issue of *Science in Context*, edited by Henderson, and devoted to the study of relations between art and science. Henderson makes plain from the start that the focus of the edition will be on historical relations, and the historiography of such relations, but she notes that there exist a variety of other approaches. These alternative methods are predisposed to a search for commonalities between art and science, and work mostly from within the fields of aesthetics and psychology. Notions of elegance, beauty, form, *Zeitgeist* and connection are shown to be the animating principle of many such theories, and Henderson is inclined to view these searches sceptically. She describes the discussion of modern art and science as "haunted and therefore skewed" (Henderson 2004, p430) by the supposed Einstein-Picasso connection. How we should relate

Picasso to Einstein, or cubism to early 20th century physics, Henderson implies, needs careful historical analysis[61], rather than grandiose but un-illuminating metaphors. For example, Henderson reminds us not only that while relativity theory was not popularised in France in time to have any direct impact on Cubist painting, such popularisation would be one – mundane – way in which influence could travel.[62] She writes:

> We will never know exactly what an artist was imagining, but a recovery of the science readily available to the public – whether written by scientists themselves or by science writers – is vital in order to establish the parameters of what was possible at a given moment. This popular literature, preserved primarily in newspapers, magazine and popular books, is a critical interface between art and science (Henderson 2004, p437).

As an example of the style she recommends, Henderson gives favourable mention to Martin Lloyd Jones, who inclines towards explanations of 'cultural diffusion' in terms of multiple forms of communication commonly used by artists: books, articles, academic lectures and seminars, art galleries and magazines.[63]

Detailed analysis of engagement diaries, reading, publishing trends – as well as actual scientific announcements - figure strongly in Henderson's method. Her scepticism of the utility of grand cultural metaphors (the *Zeitgeist*), or of mechanist metaphors (*influence*,

[61] Henderson's specialism is precisely such analysis (Henderson 1988).
[62] Henderson is able to show that popularised accounts of X-rays, coming right at the beginning of the 20th century, are far more likely than Einstein to have had an impact on Picasso (Henderson 2004, p448).
[63] Henderson does not mention that Lloyd Jones also coins the term 'hyperphor' for certain concepts he considers particularly liable to travel between art and science. Examples of hyperphors are: 'waves are particles'; 'visual form is redundancy' and 'architecture is a language'. Lloyd Jones recommends that we should not look for common denominators in art and science, but should instead "get on the trail" of hyperphors. That trail, we should infer, is not to be found in grand theories, but in records of meetings, conversations, and reading matter (Lloyd Jones 1983, p158).

connection) is complemented by the social historians' desire to root significant events in the myriad phenomena of ordinary life. For our purposes, then, Henderson encourages us to look at the ordinary cultural interchanges of scientific life as a possible route to understanding art-science collaborations. Henderson's 'exorcism' (Topper 1990, p299) of the *Zeitgeist* in favour of a detailed socio-historical analysis of cultural processes signals a precedent for the present study of examining art-science collaborations in terms of how they fit within the scientists' ordinary working culture. Henderson points out the possible merits of so grounded an approach[64] by providing the following, precautionary, quote: "*Universal mind* would be the moving force behind our *Zeitgeist*, speaking through the works of revolutionary right-brained, intuitive artists first, and later through left-brained, visionary, rational physicists" (Shlain 1991 p430, quoted in Henderson 2004, p429).

For Henderson, artists' agency is important. That is, artists are not simply passive vehicles who respond to social forces: they read books, they choose who to meet, they travel. She sees her method as in opposition to some trends of scholarly analysis: "... despite poststructuralist arguments to the contrary, we can indeed recover to some degree an artist's basic intentions" (Henderson 2004 p437).

In promoting the methodological virtues of seeing artists as active agents in their expressive development, Henderson mirrors a similar debate in the sociology of scientific knowledge. Indeed, her investigation of the theme, for example her critique of Mermoz' (1983) venture into discourse analysis, criticising it for too credible an approach to Picasso's protestations that he never read any science, echoes very precisely Shapin's (1984) caution that, *contra*

[64] Fuller (2007, pp173-175) provides a cautionary note to such 'grounded' projects, seeing them – at least in the context of social constructiviism – as preventing scholars from taking any normative approach.

Gilbert and Mulkay (1982, 1984), discourse analysis has its limits. That is, for Henderson, detailed cultural exploration can supplement discourse analysis, and together provide a way to understand the interrelations of art and science.[65]

If Henderson helps us develop a robust and down-to-earth approach, our second leading art historian, Martin Kemp, provides my enquiry with important philosophical guidance. In the pages of *Nature* magazine, Martin Kemp has been able to give ideas about art and science unusually systematic elaboration. Most of his columns are historical and they amount to an extraordinarily broad and confident scholarship. Kemp does not rely only on detail however; he is prepared to theorise, and several of his abstractions are worth exploring. For example, in his (2005) contribution to the *Nature* special supplement on science and art, Kemp approaches the issue of what concepts can be successfully used in analysing, and forging descriptions of, the art-science relation. As we have seen, while practitioners might be bold on the matter, academics are far more cautious. Kemp warns that "... to generalise about the relationship between art and science is not so much hazardous as impossible and we serve any enquiry into art and science badly if our criterion is superficially the influence of science on art, or the influence of art on science" (Kemp 2005, p308).

[65] Another methodological stand is Jones and Galison's (1998) scepticism of the academic value of investigating the art-science divide in the first place. In their *Picturing Science, Producing Art* Jones and Galison note their desire to move beyond the 'binary economy' which sets up parallels between the two types of activity. Their observation that "...there is a history to the perception of difference between science and art, and a parallel history to the attempt to unify the two" (Jones and Galison 1998, p2) is an important reminder that art-science scholarship has its own traditions. One such tradition might be the interest in doing apparently illimitable work on boundaries. Jones and Galison's examples of perhaps ill-founded questions include: Do the alligators that hang from the ceiling in the late Renaissance cabinet of wonders at Wurms form part of the history of scientific classification, or part of the history of aesthetics? Jones and Galison smartly replace the endless enquiry into whether art and science are incommensurable realms of knowledge with for them a much more promising project: their investigation will be into the conditions under which objects become visible in culture, and in what manner are such visibilities characterised as science or art. They see investigations of the binary economy as likely in the end to be an investigation of ones own position, with thoughts about the other being in essence a way of thinking about one's self. The result is a series of mesmerising reflections that capture the mind: soft versus hard, intuitive versus analytical, inductive versus deductive, visual versus logical, random versus systematic, autonomous versus collaborative, art invented versus discovered.

Kemp is prepared to offer some basic signage however, and in doing so suggests approaches usefully distinct from the recommendations of Henderson. Kemp, for example, seems on occasion more of an advocate of old-style epistemology, echoing Wolpert (2002). He writes for example that a work of art always remains open for interpretation, while science "... embodies testable content in an unambiguous way"(Kemp 2005, p309). On occasion Kemp cannot resist the temptation to emphasise the virtues of 'the other side', as when he finds himself "...happy to tell my colleagues in the art world that the graphics of modern science, as represented in *Nature*, made much of the fare in art magazines look tame" (Kemp 2000, (v)).

But Kemp provides a more enduring analysis of art-science relations. Kemp is clear that artists and scientists are capable of working with each other, or at least with ideas from the other's field; there is something they have in common that, on occasion allows or necessitates this common cause. The concept Kemp favours is 'structural intuition', meaning resonances between innate mental structures and patterns of organisation in nature that can be shared by anyone trying to understand the material world, a project in which artists and scientist are pre-eminently important.

A second concept Kemp introduces into the debate, perhaps of more immediate importance for this study, is that of reductionism. His query is this: if it is the case that artists and scientists do indeed sometimes share 'structural intuitions', how does this square with artists' interests in the visual and the macroscopic, and scientists' tendency to locate their site of interest in the molecular and the invisible? Kemp sees the structure of nature as indeed organised in layers; he suggests that complexity can be contrasted with the molecular; and he notes that until recently "...any science must necessarily strive to operate in a reductionist manner if it is to be seen as genuinely scientific. This requirement no longer seems so clear cut" (Kemp 2000, p180). Comparing Richard Dawkins' (1991) account of genes with Ian Stewart's popular work on chaos theory (Stewart 1998), Kemp wonders whether his own arguments about visual connections between artistic and scientific visualisations might not work better under Stewart's regime of complex interplay, than with Dawkins' style of science (Kemp 2000, p180; 2006, p157).

It is not that Kemp is extolling holism over reductionism. He sees as philosophical and ethical the question which 'gateway' we take to enter the system. Nor does he argue that visual language inevitably discerns only emergent structures, and ignores the reductive. But he can be understood as implying that for the artist the former will be the more natural site of investigation.

Kemp's arguments are broad-reaching, apparently dealing simultaneously with the metaphysical, the cognitive and the structuralist. They don't gain traction straightforwardly on the detail of contemporary art-science collaborations. In fact he has not, to my knowledge, interpreted an actual art-science collaboration in these terms. Yet in my study I frequently interviewed a scientist whose work was molecular and reductive; they were focussing their gaze on a tiny corner of a system, while meantime the artist was working with the whole of that system. An example would be the neurophysiologist Nick Davey and the dancer Kitsou Dubois. Frequently too the scientist would talk about the perils of over-specialisation, of the need to see a bigger picture. In meeting those desires, as I shall show, the artist took on a significant role.

When philosophers of science discuss reductionism, they usually develop two forms of argument. The first can be labelled 'ontological' the second 'practical' (Webster 2003, ch2). The ontological account is that the drive to explain nature in terms of its components is justified and necessary because, of all kinds of things, the most 'natural' are to be found at the atomic level. Opposition to this argument comes from the stance that, ontologically, the higher levels of phenomenon, such as a tabby cat, or an ecosystem, are natural kinds as real – as basic – as atomic particles (Dupré 1993). When working scientists do discuss the issue, they adopt a pragmatic approach: in terms of theory (for example genetics) and utility (pharmacology) an orientation towards molecules remains a proven strategy (Bock 1998).

As will be seen in the interview analysis chapters, the scientists develop interestingly diverse – and conflicting – accounts of the scientific method. The value and limits of reductionism is also a philosophical topic that the scientists discuss. Once again, the accounts are complex; once again, the accounts integrate within the interviews with the scientists accounts of the collaborations.[66] We shall see Kemp's point about gateways to the system confirmed, but also modified. The scientists do value their reductionist skills; but they want to maintain a genuine engagement with more holistic interpretations. Not only are they clear about this; they link their involvement with an artist as helping them achieve this balance.[67] Kemp is reticent about how he thinks an artist might encourage the holistic view in general, and he certainly makes no comment on how artistic practice could impact on a scientist's level of engagement. Yet art-science collaborations invite a rather simple and direct explanation. The artists whose work forms a subtext to this thesis are working with biologically-meaningful concepts, but

[66] For example, see the interviews with Tony Holder, Alf Linney and Mark Miodownik.
[67] See for example the interview with Richard Wingate, whose interest in, and professional use of, evolutionary theory, metaphysics and visual culture were all – by his account – stimulated and supported by the work with the artist Andew Carnie.

which find their visual expression through representations of the body, the whole organism, and consciousness.[68].

In summary, Kemp's metaphysics, especially his interest in reductionism, suggests scientists could find in artistic projects a perspective for re-balancing their investigative style. Linda Dalrymple Henderson, however, brings to this study the complementary idea that social science or historical methodology is important in representing the art-science interface. That methodology, we saw, involved painstaking attention to (for example) the popular science journals an artist might be reading or discussing.

The view from 'science studies'

In this literature review I make only brief mention of the philosophy and history of science. This is not because I think the subjects unimportant. On the contrary, they are central to any project that will question the principle that science cannot itself be affected by artistic practice. This importance I take to be best construed as methodological: the idea that actual scientific activity can be studied is arguably the most important and enduring aspect of Kuhn's impact (Rouse 1987, ch2) Most of my discussion about the philosophy and sociology of science I therefore hold over to the methodology chapter, where my strategy is two-fold. I will firstly set up a broad theoretical platform by selecting what I see as the most

[68] Examples include: Annie Cattrell's 3D Perspex brain scans of Annie Cattrell (collaborating with neurologist Mark Lythgoe); Dorothy Cross's video images of swimming jelly fish (working with the physiologist Tom Cross); Kitsou Dubois' weightless dancing (working with neurologist Nick Davey and bio-engineer Anthony Bull) and Deborah Padfield's images representing bodily pain (working with consultant physician Charles Pither). Each of these projects can bear many interpretations. I would place on them the following possibility: through experiencing artistic engagement with a higher ontological level, the scientists' approach to the scientific object is newly calibrated. For project descriptions of Cattrell, see Gere 2004; for Cross see Arends and Thackera 2003, pp16-61; for Kitsou Dubois see Ede 2000, p62;for Padfield see Padfield 2003.

promising elements of the post-Kuhnian philosophy and sociology of science. Then recognising that I need a finer resolution for dealing with the further reaches of my interview analysis, I turn to ideas associated with social epistemology, hermeneutics and a possible 'cultural studies of science', a nexus of interests commonly associated with Richard Rorty, Joseph Rouse and Steve Fuller. These authors set up a programme for studying scientific belief that I believe is fruitful for the current study. In particular, I suggest, these authors help me overcome the rather glaring problem that my chosen theoretical background, the sociology of scientific knowledge, can be characterised as notably internalist, remaining preoccupied by 'old' issues: realism, the justification of the belief that scientific belief is a representation of nature, and the mechanism by which society and its values might impact on scientific knowledge. These intellectual tools help me interpret many aspects of my transcripts, especially the sections where artists and scientists are working together. When however the discussion turns to the cafes a scientist likes to read his journals in, or the way an arts grant effects his relationship with his colleagues, then SSK seems to falter. The analysis needs a broader, more political, more *literary* style – something I believe is pointed to by the three philosophers mentioned.

Meantime in this literature review I restrict my treatment of philosophy of science to the work of two sociologists, Michael Mulkay and Karen Knorr-Cetina. Specifically, their work encourages reflections on the extent to which the concept of 'the scientific self' might help us understand the art-science collaboration. For it is impossible to read the transcripts and not develop a strong sense of these scientists having a commitment to themselves as having agency in their work, as being potent controllers of what they do. At all points in the interviews they discuss themselves, the unusual actions they take, their difference from the others. 'Self', in other words, is powerfully present. We saw that Henderson recommended that anyone trying to understand the art-science interface of the 1920s would do well to find out what artists were actually reading and talking about – the implication being that artists are not simply in the grip of massive and indefinable forces, but have an active self that makes decisions about styles of working, and deliberately locates and integrates 'influences' from the wider culture.

I have suggested that Henderson's plain words are thought-provoking for the current study. If scientists' intentions can be written into an analysis of the workings of art-science collaborations, then the transcripts gain immensely in utility. As will become evident in the last section of this literature review, texts on the nature of scientific collaborations are themselves beginning to take seriously the role of the scientists' values and intentions (Hackett 2005). Within the sociology of science literature however, it must be admitted that the status of 'the scientific self' is tenuous. I leave a fuller analysis of this paucity – and a possible solution – to the next chapter. However, so as to sketch out a preliminary account of why an account of self might be important, let us turn to the work of Michael Mulkay and Karen Knorr-Cetina.

Mulkay interprets scientists' discourse as radically discordant. In their classic text *Opening Pandora's Box* Mulkay and his co-author Nigel Gilbert show how scientists, discussing their work and that of competitors, employ two 'repertoires'. The 'empirical repertoire' is the style of discourse used when a scientist portrays his work as a true account of nature, guaranteed as such because the proper objective methods of science have been followed.

Mulkay finds however that the uncomfortable fact of a disagreement with a competitor about what theories and models are correct forces the use of the 'contingent repertoire'. Faced with divergent accounts, the scientists were shown to explain their competitors 'mistakes' as a result of a failure to follow the proper scientific method. For example, the competitor might be condemned as too committed to his own theory to judge the evidence fairly, or he might be accused of being a sloppy worker who has failed to do his background reading (Gilbert and Mulkay 1984, ch4).

One of Gilbert and Mulkay's points seems particularly important. It is that scientists' discourse is a highly unstable mix of these repertoires. The instability is caused by the incompatibility of the repertoires. Thus even within a single paragraph drawn from one scientists' interviews, a tension can exist between the way the scientist accounts for another's 'erroneous' view as caused by the intervention of social or personal factors, while recognising that the other scientist, in symmetrical fashion, may argue in precisely the same way.

In *Opening Pandora's Box* the scientists are anonymised. The writers eschew completely any tendency to 'narrativise' the scientists' contributions, for example by giving any extended biographical information that sets the transcript excerpts within a detailed personal context. The authors are committed to the value of discourse analysis, and wish to generalise their findings. Their excerpts are tools for the wider understanding of science, not anecdotes about a set of scientists working in the 1980s. Yet one could adopt a different approach. The discordant and fractured accounts offered by the scientists might have been more fully interpreted as particular scientists actively trying to make sense of divergent beliefs. It is this kind of active voice that is found in this study's transcripts, where scientists put forward notions of autonomy and free will as part of their attempts to explain their involvement with art. As with Mulkay, the discourse of my scientists is unstable, often wrestling with the empiricist arguments they judge will secure the boundaries of science, while also allowing that 'artistic reasoning' might still have a role in scientific work. Yet it is hard to see, in my transcripts, a precise mirroring of Mulkay's two repertoires. Instead, I suggest, there may be many.

This literature, and my own early interviews of scientists, convinced me that discordances and ambiguities might well be expected in a survey of scientists' views on art and science. Gilbert and Mulkay criticise the tendency for trying to tell a single story, a criticism sometimes reasserted (Traweek 1988, p12; Rouse 1996, p258). While Mulkay and Gilbert of course were not addressing the question of art-science relations, *Opening Pandora's Box* is suggestive for its account of how scientists choreograph and reconcile apparently contradictory elements in the way they understand and represent their work.

The second writer is Karen Knorr-Cetina. Her two main texts, *The Manufacture of Knowledge* (1981) and *Epistemic Cultures: How the Sciences Make Knowledge* (1999) are significant ethnographic studies of laboratory life. Their first importance here is that their social constructivist outlook includes the recommendation that understanding scientific culture will require a gaze that is highly sensitive to the local situation. Her approach echoes Henderson's call for attempts to understand art-science instructions to avoid grand narratives or a single theoretical approach. Knorr-Cetina contrasts 'frigid' with 'sensitive' methodologies; the latter, preferred approach, recognises that "we have to look hard, and we have to adopt an approach which gets us close enough to the phenomena to allow us a glimpse of their true character" (Knorr-Cetina 1981, p17). Sensitive methodologies, those likely to provide plausible and coherent accounts, are engaged rather than detached; in contact rather than removed; interested rather than disinterested. Given that the analysis of words figures so strongly in this study, it is worth quoting Knorr-Cetina at length, as she debates the role of objectivity in the social sciences:

> Of course, such disengagement is part of a deliberate strategy of non-interference presumably designed to guarantee the very neutrality we have been talking about. But this neutrality is built upon the questionable assumptions that the meaning of utterances can be taken at face value among speakers of a language; that they do not depend on the pragmatics of concrete situations; that there is no temporality of meaning, and that the observer can reach, preserve and transmit an understanding from a distance as well as from a perspective close at hand. In short it assumes that intersubjectivity[69] can be safely presupposed and need not be worked at through concrete interaction (Knorr-Cetina 1981, p17).

My early conversations suggested that as a researcher I needed to note as significant these scientists' agency in deciding to collaborate with an artist. We can see in the excerpt above from Knorr-Cetina that taking such agency seriously might well be part and parcel of of her 'sensitive' and considered methodology. Yet just as I have to re-cast Gilbert and Mulkay in order to find the active self in their key findings, so it must be admitted that Knorr-Cetina's

[69] Intersububjectivity is at root a matter of dialogue and is a concept much enriched by the Cambridge philosopher of science Mary Hesse (Hesse 1980). Her concern was that the – to her, convincing - attacks from Hanson, Kuhn and Feyerabend on the notion of objectivity, in part dependent on the accepted concept that scientific theories are underdetermined, and that choices between competing theories can never be rationally made on the basis of evidence alone, would lead to too complete a deconstruction of the validity of scientific knowledge. Following Habermas, Hesse suggests 'a model of dialogue' where scientists compare views, and judge theories for their 'empirical reach' and according to how they function together. It is far from an anarchic, or radically relativised, epistemology. It functions as an orderly, post-positivist retreat, a line in the sand that would maintain some defence of science's Enlightenment dream (that science is uniquely able to furnish us with reliable knowledge), whilst acknowledging the clear defeat of the position that science is simply an accurate representation of nature. Intersubjectivity is the process whereby careful communication between professionals, and the workings of institutions of integrity, combine to produce, if not an objective account of reality, at least a version that has been scrutinised and checked by a number of experts, and so is clearly more than simply an opinion.

analysis is by no means dominated by a sense of the scientific self. She proposes that a good way to understand the production of scientific knowledge is to imagine an architectural, or patterned, aspect to scientific research. That is, the scientific process involves networks of activity and communication that extends far outside the laboratory, and which can be named 'the epistemic culture'. Thus, rather than re-asserting the idea of social processes bearing down on scientists, who then express these forces in the form of scientific knowledge, Knorr-Cetina prefers to examine the laboratory for the way it might reverse the process, acting as the core of an 'epistemic field' that fashions, or 're-configures' surrounding space in its own image. By suggesting the laboratory sets up a kind of epistemic 'field' she supports the concept of the scientific self by repudiating the strong programme's 'mechanical' vision, which sees complex social forces finding scientific expression by 'pushing down' on scientists. Yet her descriptions of the epistemic field makes great play of the importance of the laboratory's artifice and distortion, which reconfigures all in its path, and therefore negates the humanity of the scientists within, and the artists who pass by. Further treatment of this paradox I leave till the next section.

IV. Collaborating in science

Finally I turn to the growing literature on scientific collaboration in general. Much of it is to do with management and policy and is less relevant here. But some concerns the psychology and sociology of collaboration, and links therefore with the themes of the current study. In particular, the literature suggests that the practice of collaboration is far from a straightforward exercise in sharing expertise, equipment and finance for mutual gain. The social aspects of scientific practice ensure that ordinary human psychology, and its interpretations of a developing project, can hardly be ignored in any study of collaboration.

Laudel (2001) is an example of work aiming to classify science collaborations and to reveal the general mechanism of operation. The emphasis is on collaborations as modular, as exchanges or transfers of goods and information; I interpret Laudel as taking a cybernetic view. A second paper to be considered here, Lee and Bozeman (2005), is useful as a corrective to the view that collaborations are always highly productive. They find that although the scientists who collaborate are also those who have published the most papers, collaboration looks less productive if published papers are fractioned according to the number of authors (that is, if ten names appear on the author list, the paper counts only a $1/10^{th}$ of a single-author paper for each of those scientists). Lee and Bozeman comment also on the transactional costs of collaboration. They point out that though it is conventional to regard science collaboration as both necessary (for the sharing of equipment, money and ideas) and as a good (co-operation is a classic value in science), working with other people from other departments, institutions, and countries presents considerable and varied difficulties.

The third paper I consider, by Ed Hackett (2005), extends the argument of Lee and Bozeman so as to consider in detail the transactional costs of science collaboration. Whereas I described Laudel's analysis as cybernetic, Hackett's work is better cast as reaching towards psychology and sociology. He is alert to the dysfunctionality of collaborations. Importantly for the current study, he has developed his ideas by interviewing scientists.

I should explain that none of this work considers art-science collaborations. Further, these three papers are not discussed here as an attempt to survey the literature on collaboration.

That literature, small but growing, is for the most part centred on metrics and on management issues, and is published by management journals. But these particular papers are useful for two reasons. The first is that the relations I study in this dissertation involve scientists very used to collaboration – this is made clear by the transcripts. Thus Laudel's basic description of how collaborations work might be useful; perhaps the art-science collaboration is best understood as clearly distinct, an anomaly in the scientists work. Perhaps however the art-science collaboration, and the science-science collaboration have much in common. And there are other possibilities too: that for example the scientists who work with artists are helped by being already skilled at collaboration, or at least are experienced in it.

The second reason for looking at – and counterposing – these papers lies in the thrust of Hackett's work. For as we shall see, Hackett is driven by his interviews to make good use of words like tension, paradox and ambivalence. As I have attempted to show in this literature review, terms like these seem common too in comment on art-science collaborations. The comments of Byatt and of Ede; of Wolpert and of Carnie, and those of Thomas Kuhn, could well be construed as being paradoxical. Wolpert writes that art and science can inter-relate, but the relation must be of a certain type. Ede describes artists as in need of the scientists' courageously objective curiosity in nature, but admits that artists might have a role too in the scientific project; Carnie's project 'Magic Forest' is a work of art rather than science, but he sees scientific research as standing to gain from artistic input.

Moreover I knew from many of my initial conversations that tension, obstacles and ambivalence were central features of the art-science collaboration. In face-to-face conversation the tensions emerged. Hackett's work makes it easier to explore these issues as a strategy for understanding the art-science collaboration, because it deals with ambivalence.

It also shows that the art-science collaboration cannot easily be distinguished from the usual science-science collaboration simply by claiming the latter is an efficient technical mechanism for maximising financial and intellectual resources.

Laudel examined 57 research groups in Germany using questionnaire, follow-up email, and analysis of metrical data. He takes his first task as building a classification. The first distinction, according to Laudel, is between collaborations that are "independent from a specific research process" and lead to "mutual stimulation"; and collaborations that are related to a "specific research process". The language is revealing. Mental stimulation is contrasted with "... a specific research process". Immediately we see the deployment of a split: there is scientific collaboration that has a specific direction; and there is scientific collaboration that has no such constraint. Its goal is "mental stimulation".

How might our art-science collaboration fit in here? Perhaps in the first category. Did not the Wellcome Trust describe how the "...SCI-ART initiative was launched, and has continued to be run, in the spirit of an experiment – with those most closely involved with its initiation having little idea of what outcomes it would bring?"[70] On the other hand, the EPSRC, through funding its art-science research networks, has generated discussion about how such collaborations need strategic planning in order to take advantage of the interests and expertise of members – as can be seen with the digital music research network [71].

Leaving aside this issue – and possible discussion of the merits and feasibility of such a distinction - it is clear where Laudel's own attention lies. It is the second type of

[70] Arnold, K. *Sci-Art: Partnerships in Science and Art* The Wellcome Trust (1998).
[71] http://music.york.ac.uk/dmrn/roadmap/support_and_development.html

collaboration that dominates Laudel's account, perhaps because, through its having "... a specific research process", it will be more tractable to social science investigation. Here, further taxa emerge: importantly, collaborators' input is either creative or non-creative; if the latter, then it is specially produced for the partner and is a service collaboration. Perhaps the collaborator is merely transmitting information to his partner, or providing access to coveted equipment. Here then the collaboration is not classified as creative because, we must infer, there are no psychological or institutional problems in "... merely providing ... an already existing resource" (Laudel 2001, p768).

It seems clear that Laudel's taxonomy, though simple enough, can hardly be taken as anything other than a very rough map. Each of its categories raise questions about their legitimacy. For example, what might be the relation between work independent from a specific research process, and a contribution that is related to a specific research process? Can a scientist's professional skill be so readily divided? In Laudel's scheme there is a similarly bold split between creative and non-creative work.

Laudel's basic scheme then is that the collaboration can be understood as a series of transactions. Some of these transactions are termed creative because the goal is shared and the task has been divided appropriately, others non-creative because they simply involve – for example – the lending of equipment, putting some specimens in the post, applying some statistical expertise on a colleague's data, and so on.

Leaving aside the problems this clarity of division might present when matched up to real laboratory practice, it is worth nevertheless noting again Laudel's reservations about 'mental stimulation'. Remember that this term has been kept out of the bulk of Laudel's analysis. In fact it compares to other aspects of collaboration, such as equipment sharing, as essentially beyond analysis. It might well be the same phenomenon that Arends referred to in her (2003) attempt to locate arts relation to scientific thought; it resembles the creative zone of early hypothesis formation that Popper developed.

Yet having put this intellectual activity beyond his orbit, Laudel brings it back again. He quotes from Maini and Nordbeck (1973, p192):

> As with Watson, so with Brenner, Crick discussed ideas and plans for experiments day after day. They never collaborated in the sense of doing experiments together at the same time... their particular research interests have been complementary. ... they interrupt each other, to continue either in dialogue or duologue, the ideas tumbling helter-skelter from Crick to be met by a relentless questioning from Brenner.

Perhaps Laudel's types of collaboration are in fact not so separate after all. He admits that "Collaboration involving a division of labour usually includes an element of mutual stimulation, because new ideas emerge in the collaboration process" (Laudel 201, p773). In fact Laudel's paper is interesting for the way ambitious demarcations between various categories are followed by a softening-up of the outlines.

Another significant feature of the paper is a shift from discussing how to classify collaborations, to issues of publication. He moves sharply from one to the other: the natural history of the 57 collaborations are ignored. Having found five types of collaboration, he immediately moves to similarly codify the rites of publication.

But now there are clear hints that the process is not always smooth, but rather is difficult and time-consuming. He quotes a postdoctoral scientist as saying that writing a paper involved "...very intense, sometimes too intense communication amongst all participants, which led to it progressing rather slowly. That is, we really [discussed] each word and each sentence."

Having attempted a neat and tidy proforma of collaboration, Laudel now admits to some lawlessness. He writes that the rules are sometimes broken; that collaborators who had done little work sometimes get added to the author list; that it is not justifiable for heads of laboratories who only comment on the research to be on that list; that, in sum, it can be concluded that while it is very likely that a publication's author list contain(s) precisely those collaborators who had contributed creative work to the research, this is by no means sure (Laudel 2001, p776).

The point I want to take from Laudel's paper is that collaborations resist easy description. He is forced in time to deploy concepts and examples that evoke the shifting psychological difficulties of intellectual co-endeavour; but the foreground is a taxonomy – a flow chart even – of categories and processes. In fact the author hints at the problem when he asserts that creativity has been studied, and collaboration has been studied, but never have they been studied together.

Laudel does not discuss what we might mean by creativity, beyond his example of Crick and Brenner's excited conversations. But he leaves no doubt that it remains an important concept in any understanding of collaboration. "I can confirm Zuckerman's proposition that social interaction between researchers is a crucial condition for the emergence of creativity... the mutual stimulation that bases on personal communication seems to be one of the most basic forms of creativity-promoting conditions" (Laudel 2001, p778). But he notes also the paradox that : "My analysis of creativity within the research process leads to the conclusion that creativity is always an individual property. In the observed collaborations a creative contribution was always made by a single individual and subsequently the contributions were integrated via interactions". (Laudel 2001, p777)

For the current project, Laudel's analysis is perhaps most useful for its provision of a partially-successful attempt at classifying collaborations. Like Laudel's collaborations, the art-science relations of my investigation can be studied for lines of communication, bartering habits, and authorial practices. If Laudel's own paper shows the difficulty of building these examples into general rules, no doubt I would find the same. Similarly, Laudel's constant leavening – or destruction - of his tight taxonomy with broad points about creativity and mutual stimulation suggests a subtext where collaboration, this allegedly central feature of modern science, resists reductive accounts.

The growing scholarly interest in collaboration was signalled in 2005 when the journal *Social Studies of Science* devoted a special issue to the topic of collaboration in science. Contributors Lee and Bozeman noted the way the benefits of collaboration are "more often assumed than investigated" (Lee and Bozeman 2005, p673) and went on to show that the truth of the matter is more complex. For example, though accepting the general analyses of publishing productivities of individuals which show that the most collaborative individuals are also the most productive (they get their names on more papers), Lee and Bozeman found that if they fractioned each scientists publishing count according to the number of co-authors, then "collaboration and publishing productivity are not significantly related" (Lee and Bozeman 2005, p693).

These authors are not inclined to argue however that the benefits of collaboration are illusory. On the contrary, the robustness of the 'normal count', which finds that scientists can certainly expect to increase their publication rate as they get more and more involved in collaborations, cannot be brushed aside. At the very least it shows that a research environment that demands collaboration does not prejudice productivity (Lee and Bozeman 2005, p695). Instead Lee and Bozeman take seriously the benefits that might not be picked up by standard metrics and speculate on the kinds of questions that might move the debate forward. As part of the methodological reflections my own project includes, it is worth quoting them in full:

> While we believe that the impact of collaboration on publishing productivity is an important research question, it is imperative that this single question should not dominate collaboration studies. One must consider how collaboration affects the *composition* of research, not just the resulting productivity. It seems likely that at least some of the content of the work done collaboratively differs *because* it is done collaboratively. Is the work different? Is it better? Would the work have even been possible were it not collaborative?" (Lee and Bozeman 2005, pp694-695, emphasis in original).

Clearly, though the thrust of this work is very different from that of Laudel, the same thought-provoking currents are being revealed. What makes a good collaboration? Do they produce "a different sort of science"? Here too it seems likely that art-science collaborations might find some resonance. For in those interview comments that risked making claims about the value of art, were they not suggesting the collaboration brings new aspects to a research project?

Finally we can turn to Ed Hackett's paper (Hackett 2005). Hackett was the special editor of the *Social Studies in Science* collaboration issue, and he is the author prepared to make central an exploration of the issues Laudel hints at and that Lee and Bozeman expose more fully. Like those other writers Hackett's analysis seems relevant to the current study. Firstly I see those tensions and paradoxes described earlier in this chapter as mapping onto the more academic suggestions under discussion now, and so provide context and research questions for my work; secondly these papers are doing empirical work, including interviewing, and so give useful background to the methodological issues of application of qualitative research to questions of science.

Questions of role, decisions over openness or secrecy, and conflicts over priority claims turn out, under Hackett's analysis, to be an unavoidable aspect of scientific collaboration, for he is clear from the start that ambivalence, paradox and tension are characteristic of collaborative enterprises. Drawing in the main on interviews with 24 scientists, Hackett is notable for the steadfastness with which he refuses to close down the analysis so as to reduce complex psychodynamics to simple relations. Suggestively, the key words for the paper under discussion here, itself titled 'Essential tensions: identity, control and risk in research' are *accumulative advantage, ambivalence, collaboration, leadership, paradox, research groups*. Within the paper, his headings include 'Creating Identity, Artisan or Manager: Dilemmas of Craftwork and Control', 'Conundrums of Control', and 'Dimensions of Risk'. For example, collaborations (Hackett calls them 'research ensembles') reveal forces that "separate the interests of leaders from members, spark competition alongside cooperation, complicate mechanisms of control, and offset decision-making with autocracy" (Hackett 2005, p788). Moreover, Hackett writes, "these are the inherent and enduring tensions that confront every research group" (Hackett 2005, p788).

Hackett discusses the manner in which on the one hand a group builds identity and on the other the way group members and lab heads keep their distance. The chief aim of a lab member is to attain sufficient 'velocity' to escape the orbit of the lab head and establish an independent career: the postdocs' interests are at odds with those of the lab head. For the latter those interests involve dilemmas between being an artisan, and being a controller. While craft and lab skills are important for the leader – to ignore them is to have their skills, and their authority eroded - for most however the withdrawal from the lab is inevitable. Hackett suggests it is a matter of no small significance: "Every scientist I interviewed could remember the moment or event that marked withdrawal from the lab" (Hackett 2005, p797).

And so on throughout the paper. At every stage the emphasis is on the dynamism, even the instability of the arrangement – the word 'equilibrium' appears only once in Hackett's analysis, and even then it is qualified by the term 'dynamic'. On the risk of the group failing to make progress, or of choosing the wrong problem, Hackett suggests there is no "such thing as security: ...it is risky not to take risks". On publication, should the group "rush to publish and claim priority, or delay and build advantage?" On the much-vaunted aspect of communality, should a group "share techniques and materials – and how much of *which* materials – or keep secrets and risk censure?" And "... a scientist must simultaneously communicate and keep secrets". Hackett concludes, "science is rife with contradictory forces that tug on its practitioners" (Hackett 2005, pp805-820).

The reconciliation of these tensions, according to Hackett, is difficult because while science becomes bigger, and therefore more collaborative, it nonetheless retains the principle of autonomy as a strongly held value (reflected in citation records, the idealisation of individual scientists, and the relation between academics and universities).

It is beyond the purpose of the current study to further elaborate on the tensions and problems of the intra-scientific collaborations investigated by Laudel, by Bose and Lee, and by Hackett. My intention in fact has been to set the scene for what now follows, namely a consideration of my methodology. For I interviewed the scientists in their workplace, and in effect asked them to position the art-science collaboration in relation to the rest of their work. In the setting up of those interviews, and in the analysis of the resulting transcripts, there were certain issues that were of pre-eminent importance for me, especially of course the question of how the scientist would assess the significance of the collaboration. As stated in the Preface, I have suggested it would be worth challenging the idea that in these collaborations the artistic practice stays out of the scientists professional practice. The issue of what would constitute evidence for less than complete exclusion strikes me as being a research question with strong links to the philosophy and sociology of science, and other areas besides. As will be shown the transcripts reveal a great amount of material similar to that taken up for analysis by Laudel, by Lee and Bozeman, and by Hackett.

V. Summary and research questions

This literature review suggests the following concluding thoughts, and research questions.

Contemporary sociology of science, for example through its manifestation as the strong programme,[72] encourages the investigations of society, and of 'interests', as causes of scientific knowledge. In elaborating those causes, the social constructivist project has interested itself in diverse fields (historiography, ethnography, social policy). It can be argued that contemporary artistic culture might also be included as another field for analysis by the social constructivist . Further, the turn towards laboratory studies arguably diminishes the status of foundational, epistemic matters in the building of scientific belief, and refreshes interest in the role of contingency, discourse, and the wider culture of science in the development of knowledge.

Research Question 1: Are there convincing examples of an artist (or artistic practice) temporarily or permanently transforming the narrow research practice of a scientist?

Research Question 2: The literature review showed that a description of a 'wider' scientific practice might include broader and diverse issues such as attitude to reductionism, and attitudes to collaboration and to scientific colleagues, and to the public engagement with science. How might working with an artist change a scientist's attitudes to these aspects of scientific culture?

In some contemporary comment on art-science collaboration there is nevertheless a noticeable commitment to empiricist views of science. It is taken for granted that the

[72] See footnote 4.

empiricist model is valid, and sets science apart as a belief system and a cultural practice. That commitment is too often used to jeopardise or veto sustained thinking into how artistic practices might interleave with those of science. While detailed reading of such sources (Byatt 2000 and Ede 2000, for example) suggests these authors allow *some kind* of intellectual traffic from art to science, the nature of the interchange is left imprecise.

Research Question 3: Given that Byatt's and Ede's epistemological stand on science has the weight of tradition behind it, will we find it also in those scientists who are collaborating with artists?

The literature review on occasion construed the texts as revealing oscillations between a position where science sits alone, and a position where its porosity to artistic practice is allowable. Some texts (Hafner 1969) are adamant we take seriously the art-science interface, but are viewed with anxiety for their dismissal of science's separateness (Kuhn 1977); others aimed to gain scholarly traction through the use of detailed historiography to root out simple lines of influence from art to science (Henderson 2004), thus 'exorcising the Zeitgeist'. As for texts on collaborations between scientists (Hackett 2005), they served to remind us that collaborations are weighted with familiar issues of human dynamics, including trust, matters of communication, and expectation.

Research Question 4: Might art-science collaborations be a means for scientists to explore ambivalences and anxieties about their professional persona, and about their institution?

Chapter 4: Methodology

I. Introduction

This chapter begins by focussing on the background philosophical issues I consider the basis of my developing methodology (Section II, 'A model of enquiry'). In particular I show how I derived a theoretical model that would help me interpret scientists' account of their work with artists. Then I move to consider the practical choices I made when settling on a way to gather data (Section III, 'The methodology'). After considering the factors behind my choice of interviewees I rehearse certain issues in the literature concerning the planning of interviews, and the interpretation of transcript material (Sections IV and V).

II. A model of enquiry

As has been made clear, SSK has been the most consistent resource for the development of my arguments about the meaning of art-science collaborations. It is a field with a well-developed sense of its own brief history, strongly inclined to reflection on its epistemological relations with a variety of disciplines, in particular with the natural sciences, with sociology, with anthropology and with the history and philosophy of science (Shapin 1995).

Science Observed is an important collection summarizing the state of play in SSK in the early 1980s (Knorr-Cetina and Mulkay 1983), and contains Chubin and Restivo's representation of SSK as a 'three ringed circus' (Chubin and Restivo 1983, p54). The first of those rings is the strong programme, the second laboratory studies, and the third is scientometrics. Chubin and Restivo suggested that diverse though these strategies might be, an important zone of enquiry was being missed. They argued that while the Edinburgh school calibrated itself in too broad and philosophical a manner, the ethnographic model of laboratory studies took too narrow a scrutiny. Their own interests, they felt, were covered neither by Latour and Woolgar's (1986) minute analysis of thousands of daily transactions as a scientific fact became constructed, nor by Bloor's (1976) imaginings of major socio-political forces finding expression in the development of the natural sciences.

Chubin and Restivo suggest therefore a fourth ring, home to an 'interpretative' approach that involves understanding the way scientists build their systems of belief within institutional and political settings. It is an understanding that would look too at the implications of scientific belief '*for those inside and outside of the epistemic community* (in terms of quality of life, the distribution of power, and so on)...' (Chubin and Restivo 1983, p60 emphasis in original). They urge an approach that takes an interest in scientists' values as being an important and active element in the generation of scientific belief. Yet though the two authors evoke the importance of taking ethical and political inclinations seriously, the analysis does not enquire in detail into the way individual scientists might act and talk and shape their practice. Instead Chubin and Restivo maintain their focus at a more institutional, political level. They note the value systems of cancer research, lament the corruption of peer review, and criticise Thomas Kuhn's "...failure to treat the social dimensions of science critically..." (Chubin and Restivo 1983, p69). In this account, then, values and may be power relations may be given importance; but the 'scientific self' remains vague and indistinct.

SSK's neglect of the scientific self, in favour of institutional and cultural factors, is revealed also in Bruno Latour's ,'Give Me a Laboratory and I Will Raise the World' (Latour 1983), a contribution to Knorr-Cetina and Mulkay (1983) that disputes the existence of a gulf between 'micro' and 'macro' studies in SSK, and attempts to forestall any possibility that Latour's interest in laboratory life might lead to accusations that he is losing touch with the wider world of science in action. If that is the main aim of the paper, it has the subsidiary intent of clarifying the degree to which Latour thinks scientists can be granted agency in the development of their practice.

Latour dismisses the idea of a gulf between 'micro' laboratory processes, and 'macro' social processes. The gulf arises from a simple mistake in interpreting his work. Latour, like Knorr-Cetina, asks us to consider the extraordinary efforts a laboratory makes to set up and maintain its categories. Those categories, though apparently locked inside the laboratory, and a creation of the laboratory, in fact find their application in society through a process of imposition. An example is the way Louis Pasteur transformed anthrax from a diffuse, locally variant and idiosyncratic animal disease into a specific but widespread malady caused by a single bacillus and treatable by a single inoculum (Latour 1983, pp143-144). 'Inside' the lab, it might be imagined, Pasteur made the vaccine that will solve the 'outside' problem, anthrax. But for Latour this distinction between 'inside' and 'outside' is misleading. The very conception of anthrax as a single disease with a single cause was captured first by laboratory science, and then spread through French society. In Latour's words, a feared but diffuse problem of rural farming became a single, preventable malady. Science itself undoes the difference between inside and outside.

The relevance for the current study is how to construe the 'outsideness' of artistic practice. Methodologically, in planning and interpreting the interviews, it matters whether we prioritise the search to find artistic practice 'pushing in' to the laboratory. This perhaps would be a stand justified by the strong programme's style of interpretation that proclaims the potency of outside interests. Might not art be just one more of those outside social factors which enlightened science-studiers take seriously as they show how scientific knowledge is constructed? Yet Latour's conception joins Knorr-Cetina's in positing the laboratory as a formidably powerful and regulatory field which necessarily transforms society into an image of itself. This would suggest that if art is to engage with science, it can only do so by changing itself – by becoming 'scientised'.[73]

Though SSK shows a sympathetic interest in social processes, it attenuates to the point of invisibility the scientific self. The casual observer who learns that a field called laboratory studies monitors the words and actions of scientists, or that Edinburgh's sociologists regard scientific knowledge as in part 'caused' by social factors operating on scientists and their institututions, might well imagine that the same scholars would also be telling us what scientists are 'like', how they act and think, how they conform, resist or are conflicted. To be fair Bruno Latour signals the issue may be important when he devotes Chapter 5 of *Laboratory Life*, ('Cycles of Credit'), to the problem. Writing that "we have as yet said little about scientists as individuals" (Latour 1986, p188), Latour admits that the way he and his co-author have not taken the individual scientist as an initial stimulus or "main unit of analysis" (Latour 1986, p188)[74] is perhaps surprising for a book concerned with the social construction of facts. Yet this is justified, he says, because he found that before a scientist was an individual, he was a member of a laboratory. He reports occasions where individuals downplay their own individual contributions, for example, with ideas being attributed to "the group thinking process".

[73] The Australian agency SymbioticA describes itself as an 'artistic laboratory' enabling artists to engage in wet biology practices in a biological science department. (www.symbiotica.uwa.edu.au/) Accessed 27.10.2007.
[74] The terminology where a scientist is 'a unit of analysis' is revealing.

Latour raises the question of how we might construe the scientific self when he asks "What drives scientists to set up inscription devices, write papers, construct objects, and occupy different positions?" (Latour 1986, p189). If the question is ambitious, the answer is reductive. To put his answer briefly, the motivation is not at all complex and can be legitimately reduced to the necessities of career construction. Scientists are embedded in a quasi-economic system of exchange or barter. Projects are furthered towards recognised ends partly by an understanding of the realities of scientometrics, and partly by such small personal services as honorary authorship or swapping materials and information. Success in this, Latour suggests, will build something of enduring value to the scientist, namely credibility: the scientist 'makes a name for himself'. For Latour however, the concept of credibility is quite narrowly drawn. It is your proven ability to do your experiments now, and your political ability to plan some more for the future.

Clearly these factors are important in scientists' lives. But as I began the interviews, I quickly found that these scientists were using their art-science collaboration to interfere with those cycles of credit. They had no desire to destroy those forms of barter and exchange, and nothing they said reduces the validity of Latour's isolation of cycles of credit as a major determinant of a scientist's credibility. I perceived however that the scientists referred to their collaboration in ways that evoked a world of subtle struggle within the institutions. Scientists accepted the clerical and narrowly technical necessities of their research life, but deployed their art-science collaboration as one way of resisting or transforming those constraints. For example the scientist Richard Wingate explained that he knew "... quite a few scientists who find it difficult to look ahead beyond the next small experiment, and actually get quite nervous about long term prospects".[75] He himself, Wingate continued, has broader, even philosophical interests about the nature of his work. These insights are credited both to Wingate's own intellectual independence, and to the 'reinforcing' and 'triggering' effect of his collaborating artist, Andrew Carnie.

Science as Practice and Culture, edited by Andrew Pickering (Pickering 1992) is another state-of-the-art collection appearing some nine years after Knorr-Cetina and Mulkay's own appraisal of the field. According to the collection's editor, Andrew Pickering, it had by now been established that a plurality of factors are at work in the production of scientific knowledge. The weighting of those factors will privilege neither the social nor the technical; indeed Pickering points out that the very distinction between the social and the technical had been challenged. Accordingly, while that dichotomy was fading away, a new and sharpened interest was developing in the understanding of scientific practice. Hacking's 1983 text *Representing and Intervening* had been the striking initial *locus classicus*. With Hacking's convincing description of science as performative, as actions by scientists, and as a daily struggle to make machines work, an analysis of the craft and local aspect of science was becoming more obvious to sociologists.[76] The old idea of some divide between 'internalism' and 'externalism' became problematic under this treatment: what could be more internal than what scientists are actually doing?

The title of Pickering's collection invites a discussion of terms. 'Culture' and 'practice' are common labels in academic study, but what do they mean in the context of science? Put most simply, by practice we mean the agreed actions of scientists as they struggle to manipulate

[75] See excerpt 67, p199.
[76] These themes can be found too in Kuhn (1970), Polanyi (1973) and Ravetz (1971).

nature; and by culture we mean the extended framing of traditions and over time. Those traditions and ways of being – the culture – dissolve into the practice; and both shift over time and space. Importantly however, both are bounded: the bounds are not fixed, but they are nevertheless regulated by tacit, and not-so-tacit, norms.

How might this work in detail? Knorr-Cetina's book *Epistemic Cultures: How the Science Make Knowledge* (Knorr-Cetina 1999) is an attempt to flesh out the ways scientific practice acquires its epistemic might. Commenting on a very inclusive list of influential texts, ranging from the earlier sociology that examined social systems in science (Zuckerman 1967, 1977 and Merton 1973), to the later literature that runs together the streams of science, politics and society (Barnes 1977; Pickering 1984; Shapin and Shafffer 1985; Haraway 1992) Knorr-Cetina wrote: "...[these authors] were not interested in how features of the social world, and more generally of everyday life, are played upon and turned into epistemic devices in the production of knowledge" (Knorr-Cetina 1999, p29). She is suggesting a role for an understanding of "patterns" of practice, regularities that pervade the laboratory, but also reach out.

Knorr-Cetina gives a detailed description of the essence of the laboratory. It is a place that "provide[s] an 'enhanced' environment that 'improves' upon natural orders in relation to social orders" (Knorr-Cetina 1999 p6). Here is meant that the laboratory, like a typical film set, is a place where a processed natural object, rather than the natural object itself, is the site of enquiry. Laboratories begin their work by making, buying or borrowing images, purified extracts, modified cells and vivisected animals; this is the preliminary work. Having rendered nature into a matrix of components, the experiment can begin. We are thus reminded that the laboratory is indeed a place of artifice; but is so preoccupied with the difficult mechanics of constructing and maintaining that artifice, as to view with extreme prejudice the wandering influences of the external world. This is where Knorr-Cetina invokes the cultural view. The laboratory is active not only in reconfiguring nature, but in reconfiguring social relations too. It cannot allow diffuse society to 'scramble' the laboratory's hard-earned order. Rather, the other way around.

This bears on the question of my methodology for the following reason. The laboratory's vigour in reconfiguring nature as a necessary preliminary to scientific work, and as the justification for its existence, we now learn, extends also to social relations. People coming into the orbit of the laboratory find themselves reconfigured too. The way work is organized, collaborations fostered, and communication entered into, turns out to be a product of laboratory culture. And in that cultural nexus, surely, could be placed the various artistic projects I've studied so far. But what is missing from Knorr-Cetina's account is the possibility that, in reconfiguring both the natural objects and the people who fall within the influence of the laboratory, the scientist himself, in reflexive fashion, is also reconfigured.

In Knorr-Cetina's eyes, laboratory 'culture and practice' demands obeisance from all who enter, and marshals too the world outside the laboratory wall, shaping all who come into contact. In Pickering's word: "Knorr-Cetina concludes ... that scientific culture is continuous with that of everyday life, in the sense that the culture of the laboratory *is* that of the everyday world, but artfully transformed and enhanced" (Pickering 1992, p11 emphasis in original). We can see a possible ambiguity of great utility to the current study. The 'artful' transformation that the laboratory embodies can well be understood as a fierce distortion of nature that impacts on all who come close, and on the wider society. But we might ask how this 'artful transformation' finds its initial structuring, and see in artistic practice some possible input – for example to a scientist's views on reductionism. It is important to note however that Knorr-Cetina apparently follows tradition in failing to give any role to the scientific self[77] If Knorr-Cetina's laboratory reconfigures everything in its path this might be a frightening prospect for the artist who wants to retain his autonomy, let alone have some impact on his scientific collaborator. Ironically then, the sociological insights of Latour and Knorr-Cetina, so centred on the rigidities of the laboratory might after all bear down rather harshly on my interpretative strategy, which gives importance to the scientist as a person active in formulating his professional world. For example, in Knorr-Cetina, there is no sense that in doing the re-configuring, the scientists are themselves reconfigured. Nor do we obtain from Latour, or Knorr-Cetina, any sense that scientists themselves have any autonomy in how they shape their research environment. Just as the Edinburgh school envisaged, scientific personnel are acted upon by huge forces. Craft and locality and culture may all be important – but they still all conspire to make the individual a redundant unit. In Knorr-Cetina, the scientist remains an absentee. His self - its shifts, its ambivalences, its desires – is missing, entirely subsumed by his scientific practice. It is an interesting aspect of Knorr-Cetina that while she greatly enriches the ways we understand a scientist as linking to, and acting upon, the surrounding culture, the scientist himself is left alone, an impenetrable monad.

Let me repeat: in my interviews the scientists' vigorous and frequent assertions of their autonomy was striking, as is evident in the analysis that follows. They themselves invited me to see their 'struggle' as linked to their interest in artistic practice. In my attempts to set up a model of enquiry that would ground my interpretation of the transcripts, positing a role for art as a resource for scientists' exploration of their professional identity seemed worth exploring.

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[77] By 'scientific self' I mean a constellation of personal attributes that make up a scientist's 'professional persona'.

Clues from Kemp and Henderson, and Chubin and Restivo, and my early experience of interviewing the scientists, led me to want to reshape Knorr-Cetina's programme so as to include the active scientist. The scientist is someone who re-configures nature; the necessities of his practice inevitably re-configure his surrounding intellectual and cultural space; in so doing, the scientist finds that he himself – as a working scientist – is reconfigured. To an extent it is a process willed by himself. He recognises the norms of his practice, and their conforming pressure, but resists them.[78]

In point of fact there is ample material in Knorr-Cetina's work to justify a fuller account of the individual, and a role for the individual in having a decisive role in shaping the artifice of the lab. For example, in her text *Epistemic Cultures* Knorr-Cetina's strategy is to compare biologists' professional culture with that of physicists. It turns out, as noted by Nowotny et al (2001, p100), that a key interpretation of Knorr-Cetina is that in the molecular biology lab, the individual, the person, is real, whereas according to Knorr-Cetina in physics he isn't. In molecular biology labs are distinct, dispersed in space. "Each has a principal investigator; projects are associated with named personnel. In terms of career, the objective is eventually to have your own lab. In sum, in molecular biology *...the 'scientific person' is thoroughly sustained*: work is assigned personally, in the form of 'his' or 'her' project, and when laboratory leaders describe a laboratory's work, it is done by running through current projects" (Nowotny et al 2001, p99, emphasis added). But what are these people doing? They are running labs, associating their name with discoveries, making careers. In Knorr-Cetina, the interest is how these people make and project an epistemic architecture. But on the detail

[78] The idea that scientists generate and maintain 'counter-norms' was discussed in the context of NASA scientists by Mitroff (1974).

of these people, Knorr-Cetina is silent.

Following from this, a task for the current study will be to examine the transcripts for signs they support Nowotny's interpretation of Knorr-Cetina's programme as showing that: "...individualism is alive and well in every molecular biology laboratory" (Nowotny et al 2001, p99). A final conclusion on the matter can be held over to the closing sections of this study. Meantime, with this 'model of enquiry' we can perceive the outlines of an epistemic culture where art takes its place not simply as something expressed by the passively reconfigured artist, but also as a foundation of the reconfigured and reflexive scientist.

III. The methodology

It will be clear from my preamble that from the beginning I had good access to scientists and artists engaged in collaboration. The informal conversations I was having were interesting enough to incline me towards the method of interview. But of course I had, in principle, a choice. Other techniques available included the questionnaire, the focus group, and archival study.

Mine is not a case study approach. The projects discussed by my interviewees are well known to me, but I never attempted to follow one through from beginning to end, nor did I try retrospectively to construct an exhaustive account. I wanted to encounter a range of scientists and artists, and in my analysis compare statements from different interviews. Each encounter would be different. But I thought that if I could make plausible links to developing theory, then I could make some valid statements. I didn't so much want to generalise, as draw out a set of key issues likely to be applicable to science-art collaborations of many sorts.

If I have adopted an ethnographic approach, I was not an 'observer participant'. Perhaps more than the science-science collaboration, the art-science collaboration is dispersed in space and time. Certainly it ventures outside the science institution. These collaborations include meetings, discussions of ideas, joint practical activities and, often, public discussions, exhibitions and screenings. Work took place in locations as diverse as an artist's home (Mark Lythgoe with film maker Andrew Kötting), a European Space Agency zero-gravity aeroplane (Anthony Bull with dancer Kitsou Dubois), and an operating theatre (choreographer Wayne McGregor in the Royal Brompton Hospital). There was no 'default' space which, though by no means the exclusive zone of activity, could nevertheless be described as the main site of activity.

Moreover, though I could have attended meetings and encounters, the sense in which I would then be an observer was debatable. These encounters were sometimes described by the scientists as unusual and difficult. These were not the robustly normal daily encounters of laboratory life, where a visitor is just one more variable to be shrugged off. My presence at such encounters – meetings described by one of my interviewees as "weird"[79] – would be inhibitory and distorting. That involvement, and its attendant difficulties and possibilities could of course be a project in itself, but no doubt for it to be meaningful I would have to restrict my scope to just one or two collaborations. My aim was to position myself as observer and commentator, taking up just the right orbit that would allow me to garner a significant amount of data about a reasonable range of projects. Ironically, I must make myself distant in order to perceive the matter in front of me.

[79] See excerpt 100, Ch 6.

Apart from the practical difficulties of finding the site of collaboration, and the methodological issues associated with entering so sensitive a field, there remained also the question of how productive such observation could be. Gilbert and Mulkay, in their defence of the importance of discourse analysis over other qualitative methods suggested that observation of a person at work will likely need to be supplemented by a conversation with that person to determine the meaning of what he is doing. If we see a person using a pipette, how can we know what he is doing beyond filling a long glass cylinder with a coloured fluid? Merely noting the transfer of fluid is a rather limited insight. Only by asking the person involved can we find out whether this is "...an attempt to refute an hypothesis, an attempt to find a new way of measuring a known variable, a routine check on the experimental apparatus, and so on"(Gilbert and Mulkay 1984, p8).

But perhaps the most important reason for choosing interviewing over ethnography was that, from the beginning, I prioritised scientists' talk. The art-science collaboration seemed remarkably oral. There are products; but without exception my scientists emphasised their meetings and conversations with artists. Those conversations seemed centrally important. My own 'pilot' conversations, and then my in-depth interviews, seemed richest when viewed as a record of developing attitudes towards science, rather than simply a record of a particular episode with a particular artist. Time and again the scientists implied how particularly interesting, how thought-provoking, it was to talk with the artist. It would be consistent therefore to make a primary goal an analysis of the way the scientists' talked about the collaboration and about science. My reading, and my early conversations, had suggested to me the idea that collaborations in general, and art-science collaborations in particular, might be one way some scientists can explore and even shift their working life. To observe collaborations might be to miss the point. Much better to ask the scientists directly.

IV. Planning the interviews

My strategy for sampling corresponds to Flick's (2002, p64) description of 'gradual sampling', and to Strauss and Corbin's (1998, ch13) discussions of 'theoretical sampling'. A first guide was that I was looking for 'good informants'. That is, they would be knowledgeable and with first-hand experience of art-science collaborations. By 'good informant' I also imply that the interviewees were likely to be articulate, and interested in a discussion about art-science collaboration.

The above guidelines are very general and basic, and can be elaborated by specifying what I *did not* attempt as I made decisions about sampling. I made no decisions based on the discipline of the scientist, or the art form of the artist. Thus I did not attempt to use sampling decisions to avoid the bias towards the biological sciences that results from the patronage of the Wellcome Trust. Nor did I use sampling decisions to produce a particular weighting of male and female interviewees.

To put it at its simplest, I looked for interviewees who I thought would 'collaborate' with me on the ideas I was developing as the project proceeded, and help me 'test' them. My sampling therefore was open, in the sense that my decisions on selection were being made even as I advanced through the field work phase of the project. This allowed me to make more penetrating my general policy of finding interviewees who were informed and articulate about matters arising from my research questions listed earlier. [80] Open sampling allowed me to act on the suspicion that I should have some input from artists, and could include 'intense cases' (Flick 2002, p68) where the interviewee seemed likely to be richly informative (for example, Dr Philip Kilner).

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[80] See page 107.

My initial experience of the field told me that there were art-science projects where the collaborative element contributed by the scientist was a matter of information, and remained – for the scientist – largely non-experiential. Useful work could be done into the way these scientists became drawn into such collaborations, and into their attitudes to an apparent role as scientific expert, rather than co-worker. In my project however I saw from the beginning that a strong element of the interview must be a consideration of an artist and a scientist involved in a *co-production.* With one exception (Mike Page) all my interviewees had recent experience of funded work with one, or a few, artists.

The projects my interviewees described were extremely diverse, and each interview necessarily 'adapted' to the particular case. For one, Mark Miodownik, the interview centred on the use of arts in the engineering training. For another, Mike Page, who had a broad knowledge of Wellcome's work in the area, and had written about the sculptor Richard Wentworth (Page 2000), it was the scientist's tacit philosophy of science that became the focus. For a third, Philip Kilner, the interview was dominated by a matter I had only dim knowledge of prior to the meeting, namely his unusual training.

I had met my first two interviewees, Mark Miodownik and Mark Lythgoe, at a talk at the ICA about 'the aesthetics of science' in May 2001. Here an attempt was being made to understand whether an arts concept had relevance in evaluating science. My third interviewee was the scientist-in-residence at the ICA, Daniel Glaser. Subsequently I selected interviewees following conversations with curators who were familiar with the field (particularly helpful were Ken Arnold and Bergit Arends from the Wellcome Trust; Simon Gould, arts curator at the National Institute for Medical Research, and Nicola Triscott, director of the Arts Catalyst).

Sometimes at meetings or exhibition openings I came across likely candidates for interview, (for example Philip Kilner and Jane Prophet). Interviewees also gave me suggestions. Neil Theise was suggested to me as a possible interviewee by Jane Prophet.

Before my interviews I undertook pilot work. Partly this was to decide on the spread of my questions; also to ascertain that the interviewees would be sympathetic and interested. Ken Artnold, Bergit Arends and Nicola Triscott had detailed experience of many of the scientists, and guided me. As mentioned, conversation with Claire Cohen made me wonder about how I might encourage the scientists to be more forthcoming. I had detailed conversations with Igor Aleksander, Richard Brown and Jonathan McKenzie (who collaborated on the Wellcome-funded Biotica Project); and with Andrew Carnie, who collaborated with interviewee Richard Wingate.

The actual process of interviewing was iterative. As the interviews proceeded, I realised that some points were more significant than I had earlier realised. The 20 interviews took place over a three and a half year period (this does not include my pilot interviews). After I had completed ten, I paused for twelve months, so as to reflect on the material I had gained. The interviews thus fall into two phases. As I moved through these two phases, my thinking about the interviews became, I think, more sophisticated. Looking back, I can detect three stages to this evolution. In time, as the project developed, I incorporated into my original scheme of questioning (1) the more complex possibilities noted below as (2) and (3).

1. What are the implications for the actual scientific research? What benefits does a collaboration bring? What are the implications for the tacit scientific epistemology? What was the course of the collaboration?

2. What are the implications, institutionally? What problems and obstacles did the scientist face? In what circumstances is the institution supportive? What is the interviewees understanding of art, or artists, or artistic practice?

3. What inconsistencies, ambiguities, silences and tensions can I, the interviewer, discern? How does the scientist describe his scientific culture? How does working with an artist impact on that culture, for example its extension in physical space? In sum, how does the scientist represent the role of the collaboration in his broader professional life?

In spite of the fact that the interviews developed over the three and a half year period, and the emphases changed, the basic structure of the interview remained the same:

1. What is your day like, and what do you research?
2. How did your collaboration begin and how and why did it develop?
3. How does the art-science collaboration interact with your normal scientific work?
4. What obstacles does the art-science collaborator face?

The first question, which often brought out a lengthy answer, was designed to settle the interviewee, and establish that I had some expertise in science myself: I was an informed interviewer.[81] It also served to imply that I considered interesting the question of how the art-science project related to this daily work.

[81] Laudel and Glaser (2004) consider that Latour's description of going into the Salk Institute wholly ignorant of what was going on around him, and remaining so throughout, was probably untrue and certainly

I made liberal use of follow-up questions. Having spent some while discussing science and the scientists' research I would move onto the collaboration. I hoped that this temporal conjunction of discussion about the scientists' normal work, with discussion of the art-science work, would induce some comparisons. Generally I interviewed scientists in their own office.

The approach I used is usually described as the in-depth, structured interview (Rubin and Rubin 2005) or the active interview (Alasuutari 1995; Briggs 1986; Cicourel 1964; Douglas 1985; Holstein and Gubrium 1995; Holstein and Staples 1992; Mishler 1986; Silverman 2005; Taylor 1989). Such authors recognise that the interview is itself a collaboration in constructing meaning (Gubrium and Holstein 2002, p16), with the interviewer taking a key role in shaping that meaning, but they explain how this does not mean that the results are idiosyncratic conversations of no great significance. They see investigative rigour as coming from the careful design of questions, and the use of follow-up questions so as to elicit further comments. Their recommendations are for a basic structure – a plan to follow – but one that the interviewer can deviate from so as to follow up unexpected leads or admissions, or to probe further where necessary. The interview, though not formal, is nevertheless not an informal conversation.

All interviewees were told that the work was for the purposes of a PhD thesis. They were told that they would not be anonymised in the thesis, but that if subsequently I published extracts or wrote papers, and wanted to include names, I would approach them for permission.

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inappropriate. Laudel instead recommends that informed interviewing is necessary to probe deeply and to demonstrate competence.

All interviews were recorded. Permission was sought at the time the interview was set up. The first four interviews were made using an audio-cassette recorder, and suffered some loss of data. The remaining were made with a Sony minidisc recorder.

Phase One of the interviews ran from May 2002 to December 2003. Phase Two ran from January 2005 to July 2005. Both phases involved ten interviews. The gap of one year between the two phases were important for allowing me to reflect upon the aim of my interviews, and to adjust my interviewing style and my questions.

V. The interviews

These interview details are arranged chronologically. The artists however are listed at the end. In each case I give the abbreviation that can be used for identifying excerpts in the analysis chapters. A brief summary of this information, alphabeticised by name, is given in Appendix I.

Phase I interviews

1. Dr Mark Miodownik, Reader in Materials Science, Kings College London

Interviewed May 30, 2002 (MM).

- PhD from Oxford in materials science of turbine jet engine alloys.
- Appointed Lecturer in Materials Science at Kings College London in 2000.
- Convenor at Kings College London of undergraduate course 'EngineeringArt'.
- Organised 'In Search of a Common Aesthetic' at ICA in 2001.
- Awarded a NESTA grant to set up a 'materials library' in 2003.

- Promoted to Reader in 2007.

2. Dr Mark Lythgoe, Director of the Centre for Advanced Biomedical Imaging, University College London.

Interviewed May 31, 2002 (ML).

- PhD in Biophysics from University College London.
- Collaborating with artists since 1994.
- Collaborated with Annie Cattrell on 'MRI sculpures' of brain activity.[82]
- In 2000 won a Sciart Consortium Production Award for collaborative project with filmaker Andrew Kötting, *Mapping Perception.*
- In 2007 appointed Director of the Centre for Advanced Biomedical Imaging at University College London.

3. Dr Mike Page, Reader in Psychology, University of Hertfordshire.

Interviewed July 2, 2002 (MP).

- Researches memory function.
- Contributor to Strange and Charmed (Ede 2000).
- Partner of Bergit Arends (curator of art-science projects at Wellcome Trust and Natural History Museum).

4. Dr John Tchalenko, Reader in Drawing and Cognition, University of the Arts.

Interviewed October 3, 2002 (JT).

- PhD in soil mechanics from Imperial College London.

[82] These sculptures, *Five Senses* were exhibited in the Wellcome Trust/Science Museum show *Head On.* They are currently on display at the Wellcome Trust's public venue Wellcome Collection.

- Graduate of the National Film and Television School.
- Flm documentarist on art-science relations.
- Joined Camberwell College of Arts in 1999 to head the Drawing and Cognition eye tracker project.
- Awarded Wellcome Trust funding for collaboration with the portrait artist, Humphrey Ocean.

5. Dr Daniel Glaser, The Wellcome Trust..

Interviewed November 26 , 2002 (DG).

- Senior Research Fellow in Imaging, at the Institute of Cognitive Neuroscience, University College London.
- Scientist-in-residence at Institute of Contemporary Arts (London).
- Produced television investigation of laboratory life, *Under Laboratory Conditions* (broadcast on BBC4 in 2006).
- Joined the Wellcome Trust as Development Manager in the Public Engagement. Development Group at the Wellcome Trust in 2006.

6. Dr Nick Davey, Senior Lecturer, Department of Neuroscience, Imperial College London.

Interviewed March 20, 2003 (ND).

- Co-recipient of Wellcome sci-art award for the project Gravity Zero.
- Research interests centred on the neuronal aspects of spinal injury and the physiological basis of rehabilitation.
- Laboratories both at Imperial College and at the Stoke Mandeville Hospital.
- Member of the Imperial College Biodynamics research group

7. Dr Anthony Bull, Reader in Musculoskeletal Mechanics, Imperial College London.

Interviewed June 19, 2003 (AB).

- Researches biophysics of human joints.
- Member of the Biodynamics research group at Imperial College London.
- Co-recipient of Wellcome sci-art award for the project Gravity Zero.

8. Dr Richard Wingate, Lecturer in Developmental Neurobiology, Kings College London.

Interviewed June 30, 2003 (RW).

- Researches movement, control and signalling of developing brain neurones.
- Collaborated with the photographic artist Andrew Carnie on installation *Magic Forest*.
- *Magic Forest* exhibited at the Wellcome Trust/Science Museum London exhibition 'Head On' in 2002.
- Co-authored 'Perspectives' article in Nature Reviews Neurobiology on history of visualisation of the neurone (Wingate 2006).

9. Dr Tony Holder, Head of Parasitology Division, National Institute for Medical Research (NIMR).

Interviewed November 24, 2003 (TH).

- Medical Research Council researcher the molecular biology of the malaria parasite.
- Long-term collaborations with colleagues in Africa.
- Author of essays on malaria for the NIMR's 'science and society' web pages on malaria.
- With artist Zarina Bhimji won Sciart Consortium award in 2002.

10. Dr Philip Kilner, Reader, Department of Cardio-imaging, Royal Brompton Hospital.

Interviewed December 10, 2003 (PK).

- Clinician and research cardiologist.
- Trained in sculpture at Emerson College, East Sussex, UK.
- Co-recipient of Wellcome Trust production award in 2003.
- Collaborated with composer Sir John Tavener and choreographer Wayne McGregor. *Amu* premiered at the Sadlers Wells Theatre, London, in 2005.

Phase II interviews

1. Dr Richard Wingate.

Interviewed January 6, 2005 (RW2)

For biographical details see above.

2. Dr Ian Thompson, Research Fellow in the Department of Oral and Maxillofacial Surgery at Guy's Hospital, King's College London.

Interviewed January 6, 2005 (IT).

- PhD in materials science from Imperial College London.
- Researches use of bioactive glass in facial surgery.
- Collaborates with artist Paddy Hartley, producing tailor-made casts for patients in need of implants.
- In 2004 Thompson and Hartley (2004) work received a £110,000 Wellcome Trust award to enable them to develop their work further as *Project Facade.*

3. Dr Mark Lythgoe.

Interviewed February 1, 2005 (ML2).

For biographical details see above.

4. Mark Miodownik.

Interviewed May 23, 2005

For biographical details see above.

5. Professor Alf Linney.

Interviewed June 13, 2005

- Researches application of computer imaging techniques to surgery.
- Collaborates with photographic artist Alexa Wright.
- Linney and Wright produce *Alter Ego*, an interactive face modelling system *Alter Ego*, an interactive face modelling system.
- *Alter Ego* tours with Wellcome Trust/NESTA exhibition *Wonderful: Visions of the Near Future*.
- Linney and Wright win Wellcome award for *Listening Room*, an interactive audio instillation, 2006.

6. Dr Neil Theise, Professor of Pathology and of Medicine at the Beth Israel Medical Centre of Albert Einstein College of Medicine, New York.

Interviewed July 13 2005 (NT).

- Diagnostic liver pathologist and adult stem cell researcher.

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7. Marilene Oliver, Lecturer and doctoral student at Royal College of Art print department.

Interviewed November 16, 2004 (MO).

- Artist.
- Works with MRI and other imaging techniques.
- Collaborates with imaging units at Nottingham University and Imperial College.

8. Dr Phoebe von Held, theatre director.

Interviewed May 20, 2005 (PH).

- Theatre director and freelance lecturer.
- PhD from the Slade School of Fine Art, UCL, on Brecht and Diderot.
- Won a Wellcome Trust research award to develop a film adaptation of Diderot's text. D'Alembert's Dream, 2005.
- Collaborated with NIMR scientists during preparation of script

9. Neal White, Senior lecturer in critical practice, post-graduate department at Ravensbourne College of Design and Communication.

Interviewed July 27, 2005 (NW).

- Artist
- Works with installations, performance and digital platforms.
- Arts residency at Wellcome Trust genome campus, 1999.
- Installation 'Clean Rooms' exhibited at Natural History Museum, 2003.

- Arts residency at NIMR (Mill Hill, London), 2004.
- Methylene blue event 'The Void' performed at Barbican, 2005.

10. Professor Jane Prophet, artist and academic.

Interviewed July 28, 2005 (JP).

- Artist.
- Work includes installations and digital prints and reflects strong interests in science and technology.[83]
- Launches in 2002 Prophet collaboration ('CELL') with US physician and stem cell researcher, Neil Theise, and with the mathematician Mark D'Inverno.
- 'CELL' interprets stem cell behavior using theories of complexity and emergence.
- Wins in 2005 a NESTA 'Dreamtime' Fellowship.
- In 2006 steps down from her posts as Professor of Visual Art and New Media, and Co-Director of the Centre for Arts Research, Technology and Education at the University of Westminster.

VI. Preparing the analysis

I transcribed using Word, rather than using specialist software (for example NUD*ST). I was unconvinced that the conjunctions and frequencies such software manipulate so effectively were in fact a key part of my work. My emphasis on finding the meaning of scientists' words would best be served through careful reading.

[83] http://www.janeprophet.co.uk/technosphere.html.

Recordings of Phase I interviews were fully transcribed. Recordings of Phase II interviews were listened to, notes taken, and partially transcribed. The transcripts are simple records made without the detailed attention to speech modifiers (pauses, emphases) typical of conversation analysis.

In section II of this chapter, I explained that I developed an SSK-based approach to the question of how to investigate the contemporary art-science collaboration. My two chapters on the analysis of results show that model being put to use. I now look further at some of the debates that mark social science qualitative research, especially research that uses talk as a resource.

On thinking about talk, consider the following three quotes:

1. "All we sociologists have are stories. Some come from other people, some from us, some from our interactions with others. What matters is to understand how and where the stories are produced, which sort of stories they are and how we can put them to honest and intelligent use in theorising about social life" (Miller and Glassner 2004, p138).

2. [The task is in] "...choosing interviewees who are knowledgeable about the research problem, listening carefully to what they have to tell you, and asking additional questions about their answers until you really understand them..." (Rubin and Rubin 2005).

3. "... sociologists' attempts to tell *the* story of a particular social setting, or to formulate *the* way in which social life operates, are fundamentally unsatisfactory" (Gilbert and Mulkay 1984, p2, emphasis in original).

The above three quotes serve to frame the issues that are at stake as I approach the task of analysing my transcripts. In (1) we have an appeal to stories – they are "all... we have" – as the fundamental element of qualitative social science research. What is important is that we know their source, and can put them to "honest and intelligent use". In (2) the writer stakes out a much more obviously realist path: we must listen to knowledgeable people in order to understand an issue. The unstated point is that, contra (1), there is indeed an understanding that finds its grounding in one reality. And finally, with (3) we find there can be no such privileged view, because a sensitive hearing of talk displays an inconsistency which points to the lack of one, privileged story.[84]

In so far as my approach is realist, I began with grounded theory. Originally due to Glaser and Strauss (1967) the theory was developed by Strauss (1987) and Strauss and Corbin (1990). From grounded theory comes the well-known technique of ordering texts by the process of coding. For example, when reading my transcripts, I might write down language/communication when I came across a scientist complaining about the difficulties of understanding his artist collaborator. There are arguments about whether all such categories must be inferred from the text, or whether it is permissible to import some or all of them, according to background reading or other experience. The compromise approach is to use a

[84] The issue of reflexivity haunts the sociology of science, because of the historically-fraught relation between the social and natural sciences. For the current study, which has as its interest the nature of relations between art and science, reflexivity lurks beneath discussions of methodology. In particular, how far will empirical methods take us? From a huge methodological literature exploring how and why sociology might free itself from positivism, an important influence for this investigation was an exchange among social geographers in the journal *Area*. Geography is similar to sociology in combining an empiricist heritage with important new interests in studying culture. The debate centred on how qualitative geographical research could acquire authority in a discipline dominated by the methodology of the natural sciences. See Baxter and Eyles (1997) for a rehearsal of concerns about qualitative research, and Bailey C., White C. and Pain R (1999) for various reassurances.

combination of theoretical perspectives which you bring to the study, and ideas grounded in data that is derived in part through our growing familiarity with the data.

Grounded theory, in its purest form, is inductive, a matter of allowing oneself to see patterns in data, unencumbered by theoretical perspectives. Rubin and Rubin suggest that grounded theorists attempt to build theory solely from the data at hand and in doing so emphasise theory building rather than core testing (Rubin &Rubin 2005, p240). In fact of course the problems of induction apply also to social science methodology. There must be few cases in transcript analysis where the analyst does not impose some pre-existing schema. For example, I was likely to be particularly alert to indications from the scientist about their views of the nature of science methodology, and their articulation of any notion that their scientific research could, or could not be influenced by their contact with the artist.

Rubin and Rubin are clear that one can get ideas for coding from your previous reading. They sanction mining the published literature, and one's own experience, in setting up codes, but clearly care is needed to prevent an established theoretical position overwhelming original 'signals' to be found in the data (Rubin and Rubin 2005, p202).

Coding is a process that occurs in two phases. There is an initial phase, where separate categories are marked in. In my work some categories were determined according to my own research interests that derived from my reading (thus I coded for reductionism), and from my reflections on many preliminary 'pilot' conversations. Others categories, factors I had not realized were important (for example, the scientists interest in public engagement), were drawn from the transcripts. In grounded theory the later 'axial' phase consists of a more sophisticated reading, with the investigator moving from simply noting categories, to a more interpretative stage where links are made between categories, and contradictions and sub-categories formulated. Over time the text is further interrogated using these arrays of concepts, and further insights emerge.

As analysis proceeded, then, examples were compared one with another, themes tested against counter-instances, and sub-themes were created. For example, in my transcripts, there were statements about the scientist preferring "to talk to an artist about my work than a colleague"; and there were statements about the importance these scientists attached to public engagement. Here, in the advanced stage of my coding, I would put these statements together, thus raising a subsidiary question of whether the artist was, in some cases, simply an audience. By the end of analysis, it was possible to start constructing descriptive narratives, integrating them with the model of enquiry, and adjusting it as necessary.

Overall I approached the transcripts in two ways. First I aimed to give an account of the 'natural history' of a collaboration: what meetings were like, what was produced, what colleagues' attitude had been. This approach I took to require a thorough reading of the transcripts, subsequently coding for significant elements, comparing them and building them into a realist account of what actually happened. But as I worked I found myself becoming increasingly aware of the elements of the interviews that seemed contradictory, or inconsistent, or which dealt not with the artists' and scientists' joint actions, but with their attitudes, as remembered and articulated in the course of the interview. It would be beside the point, or the province of another project entirely, to check the accuracy of these elements (for example, whether a scientist does or does not use hypotheses in his research). My second approach therefore was to explore the possible meanings of tensions and contradictions in the transcripts, and emphasise their significance.

Silverman warns (Silverman 2005, p121) that eclecticism opens the researcher to the danger of superficiality. Rather than facing and overcoming a possibly significant problem with one method, the researcher may be ducking the issue if he triangulates out to another method. However I argue that my over-arching aim has been to develop a rich description of a novel element of the scientific epistemic culture. This requires, it seems to me, both an attempt to give some sense of what the art-science field comprises, and an analysis of how it is represented by the scientists. An interpretative approach to the transcripts would not only reveal something of what art-science collaborations are 'like'. They would also show how scientists themselves construct understandings of their professional culture, and even how they explore that relation through a shifting understanding of art. That is, their understanding of their collaboration became a 'probe' for the scientists own exploration of their professional culture.

We saw in the work of Knorr-Cetina how social and technical aspects of the scientific life can be integrated into dimensions that project outside the laboratory but can be construed as part of the scientific culture. My interviews were constructed to delve into how art might operate, for some scientists, in those dimensions, inside the laboratory or outside, in scientific practice or in theory. In the analysis of the transcripts therefore I aim to produce an image of the complex system of knowledge production where artistic practice is just one of many streams. My analysis combines elements of a realist approach to social science qualitative research, with an attention to discourse that is most usually associated with constructivism. The sum, I hope, is a meaningful and plausible account of one little-studied aspect of current scientific culture.

Chapter 5. The analysis of the interviews (A)

I. Introduction to chapters 5 and 6

I have already described the methodology behind my interview procedures. Put most briefly *I was concerned to find how each scientist placed their collaboration within their ordinary professional lives*. In particular I was asking the scientist *to explore how, if at all, working with an artist impacted on their own research.* From preliminary conversations I could see that views on the matter resonated with tacit views on scientific method.

I used my literature review to show that ideas on scientific method are an important feature of debates about art-science relations, and gave examples of views on those relations being dependent on a defunct scientific epistemology – a factor that would seriously restrict the ways an art-science collaboration can be understood as engaging with the scientists' professional practice. My survey of sociological, historical and philosophical sources showed how those fields can provide a corrective resource for my project, even if to date there is negligible scholarly interest in contemporary art-science collaborations.

A further word is needed to explain the trajectory of this analysis chapter. The trajectory begins with Section II ('Denials'). In this section I examine the scientists' blunt accounts of scientific methodology, which I then relate to positivist traditions within the philosophy of science. These accounts are interpreted as evoking the distinctiveness of research life in relation to other areas of culture. They evidently have a 'sloganising' or ideological character. I problematise these clarities by reporting another class of statement in which the scientists admit divergences between what they actually do in the laboratory, and what they conceive of as the 'rules' of science. By pointing out these divergences I make plain that the current study is in part a discourse analysis, for instance in the way it makes meaning from discordances and ambiguities to be found within the transcripts.

My interest in discourse analysis is also seen in the next parts of Section II (parts b and c) where I focus more completely on one particular issue, namely the role of the hypothesis in science. I take this focus partly because the concept plays a key role in traditional epistemology (Sheffler 1957, Popper 1959), and therefore might be expected to figure in scientists' views of the method of science. Another reason however is that traditional views of the 'hypothesis' verge on the paradoxical. Often conceived as formal and testable propositions, essential to the proper launch of a scientific enquiry, their position at the 'start' of the scientific process puts them close to, perhaps even merging into, the inchoate world of creative, free thinking so often described in anecdotal accounts of scientific research (Mullis 1998).[85] Within the literature review I noted Arends' assertion (Arends 2003) that the 'art' aspect of a collaboration has its utility in precisely that moment of generative, free-form, speculation. My own study questions the proposition that art's utility in the scientific context is restricted to any particular epistemic zone, such as hypothesis formation, and finds significance therefore in the way these scientists are themselves so conflicted about the notion of hypothesis. Certainly they do not rush to house their artistic partner in that particular region of the scientific method.

[85] Peter Medawar, the science Nobel Laureate who did so much to make familiar to scientists the name of Karl Popper, and even the concepts of the hypothetico-deductive method, evoked this closeness by writing: '*In real life* the imaginative and critical acts that unite to form the hypothetico-deductive method alternate so rapidly, at least in the earlier stages of constructing a theory, that they are not spelled out in thought.' (Medawar 1984, p134 emphasis added).

Section III, entitled 'Encounters', starts another line of enquiry. Having reported scientists' construction of an epistemology that positions artistic practice as outside the orbit of scientific method, I now analyse other statements as revealing, unequivocally, the productive meeting of artistic and scientific practices within the laboratory. This 'strong position' is based on several – but not many - examples of actual practice, and by no means relies on a massive accumulation of data. My task is simply to argue that there exist *good examples* of artistic and scientific practice contributing to a common scientific project.

Case studies pose some well-known methodological problems. In particular there is argument over whether a case is constructed prior to investigation (for example its boundaries in time) or whether it is pre-existing in the world and await discovery and elucidation For a case study to seem authentic, it must be rich in detail. That same elaboration of 'texture' however, serves to pigeon hole the example as unique. How then can it be generalisable (Burton 2000, p216)? As this analysis chapter proceeds, with each quote linked to a named scientist, each pointing to a myriad of special circumstance, it might seem far fetched or even misconceived to seek for foundational regularities that root these collaborations.[86] My task instead is to find meaning and significance in the way these interviews alert us to certain less-studied aspects of scientific culture. In this section therefore I discard worries about case study methodology and argue that these collaborations are disparate examples of artistic practice being incorporated into the scientific project.

In spite of the specificity of my examples, it is still possible to see certain themes arising again and again. Regularities emerged in the way the scientists conceptualised and debated

[86] My overview of the field (p9) and my survey of projects (p18) contain some initial thoughts about the diversity of these collaborations.

two key topics in metaphysics. In turn these are the nature of scientific evidence (its truthfulness), and the role of reductionism in science (its validity). Just as in Section II I saw discussions of the concept of 'hypothesis' as relevant to views on the structure of the art-science interface, so in Section III I see in the scientists' philosophical reflections important clues to the ways artistic practice might, in spite of the scientists' protestations, be penetrating far into their research activity.

I have split my analysis of the transcripts into two. The current chapter, chapter 5, proceeds rather conservatively, paying attention to the great themes of classical philosophy of science, including metaphysics. The next, chapter 6, the second chapter of analysis, will be seen to be more expansive, and it is worth now making a few points about what lies in store. Chapter 6 compiles a rich display of reflections on how an art-science collaboration fitted into these scientists' professional lives. The purpose of my questions about the typical day, the shifts in scientific work, and the problems caused by the collaboration, always had the aim of forcing the scientist to place his collaboration in the context of his ordinary work. Inevitably, in the description of that 'ordinary work', a great deal of 'domestic' detail was vouchsafed to me.

These diverse accounts of the professional life of a scientist summon up a rich picture of a complex professional identity. As chapter 6 proceeds, the imperatives of classical epistemology become less prominent. Their place is taken by the more plural concerns of the sociology of scientific knowledge, with its broadly constructivist spirit. Now we move into a zone of discussion that sees the myriad, contingent and human aspects of scientific practice as together responsible for knowledge production, a way of thinking already pressed into service in my earlier description of my 'model of enquiry.

Thus, as we move through Chapter 5 and then into Chapter 6 the scope of the analysis widens into ever broader aspects of scientific culture. Under discussion will be issues such as where a scientist ties up his bicycle, which café he likes to patronize before finally embarking on his laboratory day, how often he works from home, what it is 'like' to discuss science with an artist, and why an artist might put up a white board in her studio to make a visiting scientist feel comfortable.

Inevitably perhaps, the theoretical framework modelled in Section II of the Methodology chapter (Chapter 4) begins to seem stretched beyond its original scope. The intention there was to show up the scientist's tacit views about methodology, and to suggest that the new philosophy of science's interest in practice, as experienced in the laboratory, provides a ready theoretical framework for 'levering' artistic practice into a significant relation with science.

With so many diverse issues now emerging however, this model begins to fail. I mark this problem by providing a further philosophical discussion (Section II, Chapter 6). Here I introduce a new theme, 'the absent scientist', and another philosophical resource, namely the practical hermeneutics of the philosophers Joseph Rouse and Richard Rorty. For in diagnosing the incompleteness of my original SSK model, it is its surprising coyness in dealing with actual scientists that comes to hobble its value. It was part of the post-Kuhnian revolution that the abstract theorizing of philosophy of science had been replaced by something more truthful about real science, closer to what was actually happening in the laboratory.[87] I shall suggest however that in these accounts, surprisingly, the scientists are

[87] Kuhn's credentials as a proven reliable witness of the processes of contemporary science are much debated. For a sympathetic account of Kuhn's sureness of touch, see Rorty 1979 and Rouse 1987. Chubin and Restivo (1983) and Fuller (2000, 2002) are less sanguine about Kuhn's contribution and diagnose a separatist agenda, where the 'socialness' of scientists and scientific knowledge has epistemic importance only within the laboratory.

either absent or their capacity for agency remarkably reduced. Yet, as I discuss more fully in the concluding discussion, I construe the excerpts that are the centre of this current chapter as re-positioning these scientists as *moral agents*. That is to say, they are purposefully deploying values in interpreting, shaping and executing their work.

A note on the quotations.

Excerpts from the transcripts are numbered sequentially (1,2,3....). The initials that follow each excerpt denote the name of the interviewee (MM, RW, etc). The name codes are found in Appendix I. In several cases, scientists were interviewed twice. In these cases, I indicate whether the excerpt comes from the first or second interview (MM1, ML2, etc). The number following the colon refers to the line number in the full transcript, available electronically as Appendix IV. Of the transcribed interviews, one is provided here in hard copy, (Appendix II).

In my comments on excerpts I refer to the number of the excerpt, and then the line number. Thus '3.2' refers the reader to the second line of excerpt number 3.

A general point is that almost all the excerpts that follow are drawn from the interviews with scientists. The discussion of artists' comments form a smaller part of the analysis, and are found within the last section of the second analysis chapter.

II. Denials

At various points in the interviews, in response to varying questions, the scientists gave a characterization of the nature of science. The thrust of the interview meant that on occasion assertions about the nature of science were in the context of queries over the influence of art on science.

a) Views on the method of science

Here we see abrupt dismissal of the idea that art has any relevance for the scientist's research activities. Although references are made here to foundational issues, as in 'the hypothesis-driven method of doing science' (1.2), it is interesting to see the scientific 'self' so strongly represented too (3, 2.2).

1 It [art/the art science collaboration] can't affect the scientific method. The hypothesis-driven method of doing science, it cannot affect that at all. It cannot affect that belief system. It cannot change that. ML2:49

2 In terms of whether the art has influenced my project; I don't think that it's influenced directly the day job, you know the nitty-gritty of what we do in the lab, and how that goes. TH:244

3 I mean it's one of the few places where you can say I am right, because here's the data, here's my theory, and they fit. I am right, you are wrong. How many other places in the world [can you do that]? MM2:401

4 The culture of science is the hermetic, or hermit, ascetic, who does his monkly experiments and produces fine, crystal clear science.... And that's my goal as well. I love writing a super-sparse, beautiful crafted paper which has no extra frills in it at all. RW1:396

These four quotes give a stark reminder of highly familiar representations of science. In the first, there is reference to science-as-method (1.1), which is to be understood as 'hypothesis-driven'. Lythgoe categorises these features as amounting to a 'belief system'. This is not a kind of belief system which easily shifts and adapts to passing cultural influences. Certainly it is impenetrable to art (1.3).

If Lythgoe reminds us of the classic epistemological and sociological traditions that make scientific analysis exempt from cultural analysis, Holder's term 'nitty gritty' (2.2) invites the more contemporary style of reflection that sees science as a craft, a collection of practices. Thus while Lythgoe invokes rules and beliefs as the basis of science's impermeability to artistic influence, Holder's comment helps us consider the barrier as constructed by the complexities – the nitty gritty - of daily scientific technique. When we recall that the constructivist project includes an understanding of practice as centrally important to the prising open of science to social analysis, it might seem ironic that in Holder's words, it is practice that cements the disciplinary walls. In fact Holder's words chime well with at least one sociological account of science, namely Thomas Gieryn's description of boundaries as located in practical settings, with disciplinary fences best understood as "...drawn and redrawn in flexible, historically changing and sometimes ambiguous ways" (Gieryn 1983, p781).

Excerpt 3 comes from Mark Miodownik. Discussing the quality of science, he finds significant its ability to demarcate between truth and falsity. The context of the comment is a discussion within the interview of the over-representation of men in the scientific professions. The reason for the gender imbalance, Miodownik suggested, could be found in the nature of science: he said that science attracted people who like to be right, and who see in science's method the possibility of demonstrating decisively the superiority of their own position. Miodownik implies this possibility of clarity is a 'problem' for science, but he doesn't dispute the accuracy of a representation of science where data on its own securely chooses between opposing views.

We can see in Miodownik's assertion of empirical decisiveness the roots of his own denial that artistic practice can elide with the core of the experimental project. Only science, he implies, possesses this finality. Twice the point is made that this is an important element of demarcation (3.1; 3.3).

Finally, Wingate's denial takes yet another form. Here the quote is taken from a part of the interview where Wingate explained his suspicions of the "sci-art world", and declared he would not wish to be drawn into it: "I don't want to become a sci-artist".[88] The implication of this section of the interview is that Wingate sees the art world, and therefore artists, as markedly different from science. His main statement about what categorises the scientist is excerpt 4, which focuses on the disciplined and heroic nature of the scientist, someone who is an ascetic – a monk – and cherishes austerity and simplicity, qualities Wingate found lacking

[88] See transcript p105, line 600.

in the public face of art.[89] The point resonates of course with Steven Shapin's well-known comment that science ethnomethodology "is a form of asceticism" (Shapin 1995, p312).

I suggest that these four quotes map neatly onto the mesh of developments we associate with Kuhn's original (1962) intervention, and with subsequent reactions. Lythgoe gives the traditional rendering of scientific method, the received view (Suppe 1977) or 'legend' (Kitcher 1993) that sees science as reliant upon, and significant for, its own unique set of intellectual rules. Lythgoe's comments set up too a theme of abstraction. In the context of detailed discussions about the vicissitudes of his own scientific research, and of his collaboration with Andrew Kötting, these assertions about hypothesis-driven belief systems are striking in their austerity.

However, as soon as the quotes are interpreted so simply, and grounded within the empiricist manifesto, my analysis begins to fail. For though the quotes are amply resonant with canonic views, express a version of positivism,and reduce the importance of individual agency, they have to be seen as challenged by other evidence to be drawn from the interviews. One of these challenges, quite simply, is posed by 'the fact' of the interview. What I mean is that the transcripts always reveal a person apparently taking possession of their science. In the case of Mark Lythgoe, his simpleness of vision about the scientific process was in no way matched by the developing themes of the interview, or even his physical presence. In the interview room he was an agitated and animated persona who constantly referred to the enormous stresses and mountainous difficulties of his professional life, while professing his pleasure in working with Andrew Kötting. Of course it is true that much commentary on science has

[89] Wingate noted in his interview a fondness on the part of the Wellcome Trust for 'wine and cheese'. See transcript, p109, line 733.

shown the importance of the personal touch, even while explaining how it is gradually excised from the finished product (Polanyi 1973). I nevertheless reflected as I assessed the interviews how remarkable it would be if the scientists' personalities and idiosyncrasies could indeed be exorcised from their developing projects.

If Lythgoe has removed the human from his account, Wingate does not so much reduce the role of the human as idealise it. In excerpt 4.1 Wingate conjoins the terms *hermetic* ('sealed, made air-tight') and *hermit* ('one who leads a solitary life'). Wingate thus sets up a recognisable theme or even tension in representations of science: science may be an austere and abstract intellectual enterprise, but it nevertheless requires for its execution a particular kind of person, an "...ascetic, who does his monkly experiments and produces fine, crystal clear science"(4.1). Note too the paradox of Wingate's love (a 'strong devotion... to something'[90]) for writing scientific papers that are sparse, free of embellishment, and, one knows, entirely technical.

Another perspective on the theme of isolation comes from Alf Linney.

5 You see there are some scientists who actually think art is quite destructive in this sense, I don't believe this at all, I don't subscribe to that view... I really don't see it that way at all. It's probably because they have looked and seen how science has suffered in the past. But science hasn't suffered from artists. It's been religious bigots, and Stalinism, and centuries of religious persecution which scientists might see artists as allied to.... Scientists might see artists as allied to this. I can't think otherwise of why they might think art is dangerous.

[90] Definitions taken from the Shorter Oxford English Dictionary (2003 Fifth Edition).

> It's not only artists who are the victim of this comment, its society at large. Art is just at the short end of that. AL:248

Here an important and surprising historical perspective is achieved. Linney implies that the demarcations asserted by our scientists are not merely a distillation of second-hand philosophy learnt at the hand of secondary and tertiary level science education, but have a deeper historical resonance. The references are perhaps to Galileo's house arrest in Firenze, Stalin's sponsorship of Lysenkoism, and Hitler's wrecking of human genetics through his systematic development of eugenics.

The underlying theme that unites Linney's comment, and the previous four, is the necessity to defend science from outside influence. Within the history and philosophy of science it is never an effort to interpret various epistemological positions as diverse attempts either to shore up those defences, or demonstrate their illusory nature. To reach sharply back to the origins of modern science, the foundational work of Sir Francis Bacon has very much the atmosphere of a project intent on building parapets. For not only does Bacon celebrate the utility of the new learning, and attempt to account for its mechanisms, he also provides the reader with some alarming consequences of letting the scientific project commune too closely with the chaotic humanity of ordinary life. The consequence, to put it baldly, would be the cessation of science. The four "idols", better understood as "delusions" are parables that see science as likely to be disabled should it cross the road and rejoin the world of free chatter, hotch-potch tradition, and personal vanity. These foundations of the scientific enterprise included a strong sense that science had to be isolable, and alert to the dangers of ordinary human thought and conversation.

More contemporaneously, though a reading of Karl Popper can become dominated by discussion of the validity of his 'solution' to the inductive problem, it is his vigour in establishing science as a shining exemplar of human achievement that makes him so striking a philosopher (Popper 1976). When Linney notes religious bigots, and Stalinism, we are reminded immediately of Karl Popper's own experience of pre-WWII fascism, and his diagnosis of an urgent need to defend science from the certainties of fascism. And among the many ways in which Karl Popper and Thomas Kuhn make common cause, special mention should be made of their internalism: for Popper a methodological algorithm that properly applied guarantees science's protection from dictators' scientific pretensions; for Kuhn a protective social coterie imbued with codes and practices impenetrable to outsiders. The defence of science's specialist epistemic credentials has been seen as both the aim of the Kuhnian project (Fuller 2000) and, conversely, as something that Kuhn rendered impossible (Rorty 1979). I raise these arguments to make a simple point: with so rich a tradition of seeing the powerful necessity of defending science's separateness it is no wonder that our scientists have found it easy to articulate a vision of scientific practice as discrete, while being far more spare when contemplating the possibility of cultural interchange.

It is worth noting that excerpts 1-4, from younger scientists, imply a separation, but there is no atmosphere of moral urgency. In Linney's quote (excerpt 5) a more menacing tone emerges. The terms used in connection with scientists' vetoing of artistic influence are "destructive" (5.2), "suffered" (5.4; 5.5), "religious bigots… religious persecution" (5.5; 5.6), "dangerous" (5.8) and "victim" (5.9). Linney is a senior scientist, and might well have a more developed sense of history than his younger colleagues.

His words are also reminders of David Bloor's complaint, in *Knowledge and Social Imagery* (Bloor 1976) that sociology is only allowed to study scientific knowledge when certain kinds of error have crept in. Bloor imagines that the errors under discussion are so serious as to take the method (and so the scientists) out of science. The Royal Road to Truth has been dynamited and made impassable by greed, vanity and political ambition. The damage is great enough to have gone far beyond the corrective tinkering we expect from ordinary peer review. Social causes have intervened, and the project is wrecked. And with first class science stalled, only now is there a need to call in the sociologists and the historians.

The theme of course was also explored by Gilbert and Mulkay (1984). Chapter 4 of *Opening Pandora's Box* is called "Accounting for Error" and by virtue of their time with scientists, they appear less abstract than Bloor. Thus the errors they deal with seem quite banal, certainly they are part of laboratory life. It turns out that by 'error' we mean in the first instance no more or less than a disagreement. And in the longish process by which that disagreement becomes converted into a community agreement about which theory should be adopted, there is a stage where the 'other's error' is explained for, in the absence of complete community agreement , by an accusation of social involvement.

A related point can be drawn from Latour. According to Latour (1983, p165) any strength a scientist has is entirely down to the rigidities of the lab: "[a] greedy, short-sighted politician, once in a laboratory, is going to churn out exact scientific facts, and the honest, disinterested, rigorous scientist put at the helm of a political structure... with no mistakes allowed will become fuzzy, uncertain and weak like everyone else". Latour is suggesting that the laboratory is a space whose functionality is reliant entirely on routine and regularity. For science's sake, all disruptive influences must be kept out.

It is no wonder these scientists say what they do. They are upholding a tradition. Let us now see if we discover any richer veins – any ambiguities or second thoughts – as the scientists continue their talking.

b) Views on the craft of science

If the transcripts show scientists drawing their boundaries, they also reveal the scientists struggling to give an authentic account of how they do their science. In what follows it is interesting to notice how the scientists prioritise the importance of what they do – of practice.

> 6 …brain development is a huge subject, uncharted waters. I've said this to Andrew: in terms of science, it's more exploration and cartography rather than necessarily hypothesis-driven science. And that's a big distinction. RW1:161

Straightaway we have a distinction. Whatever Wingate might say about his monkish science, he also asserts that his practice is different from rule-bound science. Pertinently for the current study, he marks the significance by connecting it to a conversation with Andrew Carnie, his collaborating artist.

> 7 I'm not sure that the thing I do is strictly speaking science all the time. I like that idea of exploration, I certainly see my particular field as being colouring in; if I look at my mental model of what a brain looks like, of how much we know, there are big uncharted areas and small parts which are intensely characterized; but across the whole of science…? RW1:552

Wingate queries here whether his own understanding of scientific practice would be transferable to other fields within the sciences. There is a strong resonance with recent work in the philosophy of science, in particular that of John Dupré and Nancy Cartwright. These philosophers have attacked reductionism for its ontological assumption that there is only one kind of natural kind, namely the atomic or subatomic particle. Instead, they argue for an 'ontological pluralism' where higher level systems studied by a multitude of different sciences are considered real and irreduceable, and therefore linked to sustainable differences in investigative styles (Dupré 1993; Cartwright 1999). Wingate's doubts also link to the strand in the sociology of science that makes much of local expertise, even to the extent of seeing every laboratory as unique (Pickering 1992).

> 8 You want to find out why a rain forest tree isn't doing particularly well because there's a coal plant next door, you pour ammonia on it - which I've done, out in Borneo, you measure its leaves, and then you say "ah well, this ammonia is not too good for this rainforest tree". That's the scientific process, but I'm not sure its science in the sense of searching for knowledge. RW1:561

The experiment in the rainforest is made to look simple and effective on its own terms, but distinct from the kind of science that is "...searching for knowledge" (8.6). Wingate is putting some distance between what he does as a scientist, and "hypothesis-driven" science (6.3).

Wingate continues the map-making metaphor with his reference to exploration (7.2): some parts of that map remain "uncharted" (7.4). Given his extended use of the metaphor, it is intriguing to see how Wingate situates his description of the scientific process. He describes a simple experiment taken not from his Kings College laboratory but from a remote tropical rainforest, the kind of place we like to associate with exploration and map-making. Wingate's choice of example places the very simple in the middle of the very wild. It is interesting to reflect on the relation between Wingate's description of the scientific process, and the way the sociologists Latour and Knorr-Cetina have made the scientific process dependent on the most rigourously controlled environments

c) The role of the hypothesis in scientific method

In this section I consider the way these scientists describe the role of the hypothesis in their work. The term is notable in being both a celebrated marker within classical epistemology, as well as a work-a-day label when scientists discuss their activity. In what follows however the term hypothesis seems laden as a symbol of outside interference. Construed as part of the questioning attitude, the hypothesis is a true component of the scientific life. Recast as a formal necessity, it becomes an irritant, a constraint.

The point gains importance when we consider Wingate's implication that the "the hypothesis-driven" method does not connect with his work with Andrew Carnie; the point contrasts with those more philosophical assertions that the generation of hypotheses is the location of creativity and the site of diverse influence; a necessarily chaotic intellectual zone ultimately transformed into valid scientific knowledge by the rules of the experimental test.

In the excerpts below the term 'hypothesis' is viewed with scepticism. Far from being a useful space for creative thinking, for example with artists, the need to deal with 'hypotheses' has become an imposition. With hypothesis, one imagines, goes the word 'test'.

> 9 There has to be a core of experimentation, and the experiments are an important part of selling your work, but personally I think I still see it as an exploration, that's the most exciting thing. RW2:270

There is an obligatory core which is experimental. Wingate exclaims the experiments help 'sell' your work. Wingate marks a distinction: he likes the word 'exploration'.

> 10 Originally when I was doing my PhD I really didn't know what, I don't think any of us knew what a hypothesis was, I don't think I'd hardly ever heard the word or other people using it, and as for actually going out and setting a hypothesis up and testing it was just beyond us, we just didn't know, we were just playing round in the lab… you know you'd have an idea you'd have a guess see if it worked or not ... ML1:146

It is interesting here to note that one could construe 'hypothesis' as 'just playing' or 'you'd have a guess'. Yet in the excerpts below the scientists find the concept of the hypothesis looms large not as creative and pleasurable guesswork but as something altogether more formal. It has taken on the attributes of the audit culture, and is now malign.

11 But now when you write a grant or whether you write a project license you actually set up the hypothesis and then you say how you are going to test this hypothesis. ML1:153

12 I feel it is more of a paper exercise and that in the reality of it we don't discuss the experiments in those terms I think somehow it seems to take the fun out of it. ML1:158

13 I just look. I collect data. It's more like natural history. I describe. I categorise....This idea of making hypotheses is kind of limiting. It's what you have to say in your grant proposals. NT:83

These discussions about hypotheses are revealing. The term 'hypothesis' symbolises the need to frame grant proposals in ways that catch the attention of the managers of contemporary science (11.1), and that meantime takes the fun out of the laboratory (12.3). The problem however is not that 'hypothesis-making' imposes a structure, for these interviewees consistently describe science as an activity requiring the severest discipline. The problem is that these scientists doubt that this is what they do. A scrutiny of their assessment of the term hypothesis suggests a suspicion of attempts to generalize scientific method. They worry that the need to describe their hypotheses in a certain form will jeopardize their autonomy.

Here we can begin to speculate about the way these scientists might view artists. If hypotheses symbolize the unacceptably managerial and institutional, might not the artist be the desired symbol of individuality and freedom? In other words, in interpreting the transcripts, a possible link begins to emerge between various suspicions of modern science, and a representation of art and artists that emphasizes their freedom.

It is enough to remember that the same scientists who so quickly gave us a demarcated view of science are now being articulate in critiquing one of the main building blocks of that view of science. Seen in that way the contrast could not be more obvious. I take this precisely as an example of the ambivalence and tension I have been discussing. On the one hand the scientist displays a method of science where the self is absent, or in a state of abnegation. On the other hand apparently these excerpts show scientists rebelling against the procrustean bed of hypothesis formation, and elaborating on their own autonomy. The hypothesis is an attack on autonomy.

Before presenting more excerpts, it is worth making a general point. I shall be suggesting that these scientists, in their interviews, evoke aspects of their work where potency lies in chance encounters, strange events, travel, and working at home. Yet they work within institutions, and describe their profession as a life filled with routine, with the discipline of hard work. The transcripts show a procession of oscillations between the strict and constant liturgies of research, and the more pleasurable contingencies of private study ("hiding away"), chance encounter and travel.

The argument now emerging is that these scientists' attraction to artistic practice is in part connected with a desire for autonomy in the way they shape their scientific work. Artistic practice is not in fact having to force its way through the homogeneous shiny surface that marks the boundary between science and everything else. That surface is not so much a barrier as a means of exchange. In a host of ways, the artistic enterprise is being drawn directly into the scientific culture. To deal with art, to find a way where artistic practice has functional value in the life of the scientist, is to challenge and push back the baleful forces of institutional science.

This speculation suffers a direct challenge in the excerpt below from Mark Miodownik. Here, all the factors mentioned above are now assembled neatly into a narrative. The process starts with free enquiry, mediated first through Miodownik's imagination (14.1), then through his contacts (15.1). In time however the formal necessities of science are to assert themselves.

First, the embryonic idea:

> 14 I just absolutely had a hunch that biology had a lot more sophistication than just genes that just turn on and off, I just had that hunch. MM:1:148

Then some informal talk:

> 15 So I wanted to talk to people about that, and see if they'd thought the same. And of course everyone had their own theories about it.... they'd all go... "yeah, it's funny because....". So you had these vague conversations...and then I said to them, we need a problem we can really test, we need a good problem. In a sense were looking for an interesting question, which is like, "does it do this?" or "will it do that?" and honing that question is the point I want to get to before I.... (GARBLED) ..... once we have the good question to ask. Once we have that, then everything else follows. MM1:150

And now the formal part:

16 Then you go "I know how to answer it, do the experiment". Essentially the scientific process kicks in. You change the variables one at a time, you do enough tests, you do a number of things, that's all easy, and in a sense boring, actually, that's the problem with science; once you've got your interesting question, it's three years before you get an answer, roughly. The first bit is great, a really interesting question, and then three years later you have this really amazing answer, or maybe you've just sparked more questions, but you have an answer. MM1:156

We can construe this account as the finding of order in the apparently contradictory elements of the personal and the objective, and between liberal-mindedness and strict deduction. In this account, the overall pattern is of an inchoate interaction of the social and the intellectual gradually but irresistibly giving way to the machinic algorithms of the scientific process. What starts off as a "hunch" (14.2.) becomes the subject first of "vague conversations" (15.4), then something more formal, for example a "... good problem ... an interesting question..." (15.5). Miodownik's account, though elaborate, is also linear. The talk, the hunches, the vague conversations are gradually replaced by a simpler format: "...the scientific process kicks in" (16.2). Essential though boring, the batteries of tests on a simple question, the variables altered one by one (16.2): this is a discourse where the open and enjoyable does indeed seem to alternate with - is replaced by - the grindingly pedantic.

Mike Page was more explicit:

17 I don't think an artist can tell a scientist how to proceed, I don't think a

scientist should be in the business of telling an artist to proceed like a scientist ... maybe they can perhaps understand each other best by sitting there thinking "what next" and requiring some sort of recombination that they both require and perhaps if they could teach each other how to attain that sort of nirvana of generation, the generative step... MP:136

Page implies through his use of the word *nirvana*[91] that true pleasure comes at the beginning, the 'generative step' (17.7): it is the period of pure thought rather than pure hard work. Artists can assist, then, with the 'generative step'. The process sounds informal, creative, perhaps sudden and unexpected, very likely taking place outside the lab, certainly away from an experimental protocol. The reference to nirvana suggests an other-worldly possibility: a desirable place of blessed thought, uninhibited by constraints and boundaries.

Here is a more prosaic example.

18 I enjoy talking to Andrew more than I enjoy talking to other scientists about what I do, that's for sure. That's a key thing, actually, that it's more interesting trying to explain what I like about science, than having a scientific chat, which is vaguely competitive and sometimes a pretty useless process. RW1:588

The conversations Wingate had with Carnie were science conversations about the laboratory specialism but there was no competitive edge. We are reminded that in the world of

[91] Defined by Collins Concise Dictionary (HarperCollins 1992) as 'final release from the cycle of reincarnation attained by extinction of all desires and individual existence, culminating (in Buddhism) in absolute blessedness'. Strictly speaking then, the analogy for a scientist would be the release from the 'cycle of science', perhaps to be construed as the dogged journey between idea, test and explanation

laboratory studies, talk is central, but not necessarily generous (Gilbert and Mulkay 1984). When Wingate talks to Carnie this is not the narcissistic pleasure that comes with finding a pliant audience. Carnie and Wingate are talking about their work, and making common cause.

Need such conversations be corralled as extra-scientific encounters, outside the research process? Or are they, as recent sociology suggests, knitted-in to the very core of the scientific process. I suggest these excerpts intimate the artist can enter the scientific worlds of technique and process.

It is good however to remember the first section of this analysis, 'Denials'. Here is Mark Miodownik dampening down such ideas.

19 (SW)Of the things we've talked about, like your daily rhythms, your teaching and your meeting with students and PhD students, do any of those things, in your memory, affect the way you do your science...?

(MM) hmm...[pause] Not really, except that I think that teaching is good in that it keeps you - as long as you approach it in that very open way, that you don't know all the answers - it keeps your mind active. MM1:192

This is a remarkable quote. Miodownik resists my leading question and insists it is not art that is a possible influence, but teaching. Teaching in the right way "keeps your mind active"(19.6). Yet according to these interviews, if a simple phrase was sought for the essential merits of the collaborations, "keeps your mind active" might be the term chosen.

I asked the same kind of question to Richard Wingate.

20 I don't think the [artists'] themes are particularly useful, because they are emotional themes and scientifically it doesn't mean anything, it just doesn't mean anything. The actual technical approach and the way we see. Its more the way we see. That's what it comes down to. RW1:533

If science is so remote a business, where actually do scientists get their new ideas from?

21 You usually see a hole, people look for a hole in the data, look for a hole in the field, you are looking around and suddenly it is really obvious that there's a space, you know, to fill in, there isn't that piece of information or there isn't that particular animal model... ML1:166

22 It's really obvious to me, I see a hole and it's exactly what happens at this conference I suddenly see, God there's a hole there and I just see it and you just wonder if you can fill it and you know it is really like the peg and square thing, God and I can just see the shape... ML1:178

Here the new ideas are coming not from chaotic encounters with mavericks, but from the fine-tuned appreciation of conference proceedings: the sudden realization that the scientific facts on display at a conference denote not only completed experiments, but experiments not yet thought of. Here, then, even the preliminary stages of idea formation are securely attached to, and dependent upon, guaranteed scientific knowledge, a process strongly reminiscent of Kuhnian puzzle solving (Kuhn 1962), where the scientist is tested by his ability to properly diagnose and cure odd twitches and problems in the prevailing paradigm.

There are echoes too with other philosophical strands which evoke a sense of scientific knowledge as having a shape, or form. Quine's celebrated paper 'Two Dogmas of Empiricism' (Quine 1951), suggests that scientific propositions are allowable through the way they cohere with pre-existing knowledge; that is to say, they acquire their meaning through their relationship with a context. To be sure, Quine's holistic model envisions the architecture of scientific knowledge as plastic and organic, in contrast to Lythgoe's 'hole and peg' metaphor. The 'ecosystem' that is scientific knowledge can undergo internal shifts to allow the establishment of new ideas. Both however affirm that scientific knowledge is not a collection of autonomous proposals, each judged independently of the other. The production of scientific knowledge is dependent on its coherence with existing models.

Lythgoe's stretching of science's reach back to its generative beginnings is accompanied, in this scientist, by a concern over the personal cost of being in the grip of so powerful an intellectual tool:

> 23 If you take on board the scientific method and you practice it day in day out that definitely changes your view of the world. All those things that I would use in daily conversation that were based on no evidence whatsoever, just a gut feeling that you know, whenever its grey it rains, or you know we're only using 10% of our brain, those things are urban myths, they are in popular culture, we use them all the time, but then I stopped using all that, and I

constantly questioned people who make statements like that. It had a massive impact on me as a person. ML2:94

24 When you stand up in scientific meetings, it's very formulaic, the way you give the talk, or even the way you write a paper. ML2:28

25 ... science makes you very, you write papers in a particular way you give talks in a particular way, it is very constrained very formulaic and actually I found, I lost a little bit of Mark Lythgoe... ML1:263

This doesn't sound an attractive fate. Here the scientist admits: a little bit of him was lost. In a sense, he has become absent. That is the cost. In fact an account like this does suggest why an artist might expect few impacts on the scientist. For that scientist is travelling to conferences, and reading scientific papers, with a view to spotting the flaws in competitors work, and sharply moving in and taking advantage of those gaps. Science is social, but its rigid competitiveness forces all social interaction to remain within the profession. Yet this portrayal, drawn from Lythgoe, is contradicted by Lythgoe himself: he is a successful scientist and a successful broadcaster.

III: Encounters

a) Introduction

In this section, the focus changes. My interest now is not so much in scientists' assertions about the distinctiveness of their work, as in statements describing the way artistic skills and sensibilities take up a role in the laboratory. I begin to stake out a possible strand of evidence. I discuss four examples of activities and practices that I consider subvert those earlier denials: they are evidence of artist and scientist finding common cause in common practice. Each example has a very different aspect but have one important matter in common: they concern practice. They do not finally refute the denials of Section II, which often relied on abstract notions about the meaning of science. Certainly however they demonstrate a theme strong in this thesis: that – given the proper opportunity - scientists can indeed articulate how science might be put into relation with art.

In Section II, I described a scientific culture sure of its boundaries, less so about the nature of how best to describe what took place within. Here in Section III I interpret a very different set of statements as suggesting artistic practice can be centrally involved in the core processes of science. Methodologically speaking, I see a revealing contrast between Section II and Section III. To put it bluntly, in the former, it is science's abstraction that prevents art from joining in. In the latter, it is art's technical practice that allows it to make a contribution.

b) Ian Thompson

My first example concerns the materials scientist and orthodontal research fellow, Ian Thompson. Thompson trained as a materials scientist at Imperial College (his original intention, at secondary school, had been to become a dentist). He was supervised by Professor Larry Hench for his PhD, and subsequently worked in Hench's team. Thompson worked on the development of bioglass, a low-calcium glass remarkable for its property, when implanted into the human body, of not being rejected. I interviewed Thompson high up in the Guys' tower, in his room just next to the waiting room of the maxillofacial clinic. At Guys', Thompson makes bioglass implants for patients needing dental and facial reconstruction. The particular nature of bioglass makes its accurate casting a vital necessity for successful surgery. It is too brittle to be mechanically ground from data obtained by scanning. It was while attempting to develop his casting skills that he met Paddy Hartley, a sculptor expert in the lost wax method of casting (though at the time Hartley was giving his attention to the development of a number of face corsets).

> 26 I needed a better skilled set of hands to make moulds to make implants that we were going to implant into patients here as part of a clinical study. So our first exchange was him helping me make implants; so the two of us stood together by the furnace and made the moulds then and there, melted the glass, pour that into the moulds, actually physically made them the two of us; my previous efforts had been OK, we'd implanted some into patients, but the response back from the surgeons was we need better shapes and smoother edges and things like that, and basically that meant better moulds. IT:18

The artist has a practical skill the scientist needs. Together at the furnace the job is done.

> 27 ... a lot of the knowledge of mould making was in the material scientists' hands from the iron and steel industry, for casting. Since that's deteriorated drastically, a lot of that knowledge has gone; but some of it had been translated into the arts community, for making bronze casts and things like that. And so that knowledge was sitting in the arts faculty and it's not until Paddy walked in the door and said this is what I do, and I said "Ahh! You're the very man I need", and brought him in. IT:31

As often in these transcripts, we have a narratival aspect: here, the artist provides a solution, and moves the story on. It is a striking example too of the way artistic practice can take its place in science. The example reminds us that in a study like this, the artist's repertoire in terms of practice must always be kept in mind. In locating the relation of art to science, we can look to the practices of both as the grounding for a link, just as we can look to their concepts and values.

Let us now consider the argument that this contribution of Hartley's is as a technician, rather than as an artist. Following Knorr-Cetina's phrase, we could describe Hartley as entering the orbit of Thompson's practice and technical needs, and being 're-configured'. Thus re-configured as a technician, he is no longer an artist, and the collaboration is better described as science-science (or engineer-engineer), rather than art-science.

This critique is fallacious because it prejudges what kind of thing artistic practice can be. It drives a wedge between "artist" and "practice". If defining art is notoriously difficult, so must be statements about what it is not. It is far from clear that any headway at all could be made in taking Hartley's skills at casting, and splitting them into technical and artistic. An analogous hesitation is common in contemporary sociology of science texts, which disparage the split between the technical and the social. Consider the possible meanings of Knorr-Cetina recommendation: "We must engage ourselves in laboratory reasoning, which reveals the scientist to be a *practical reasoner* who refuses to be split into social and technical personalities" (Knorr-Cetina 1981, p23 emphasis in original).

c) Richard Wingate

Richard Wingate also described how his artist collaborator, Andrew Carnie, brought valuable skills to the laboratory. In this quotation, Wingate refers to the highly visual aspect of his work, and his need to be able to mentally orientate complex images of brain cells, so as to understand their movement.

> 28 It takes a very strong three dimensional spatial awareness to deal with [migrating cells]. I find it very natural and very easy to think in three dimensions and rotate objects in my head but I realize when I'm explaining the structures that I'm looking at to other people, they just don't get it. That also helps, talking to someone who's also got a very clear three dimensional view – you can sketch out a schematic and Andrew can see the form in it very quickly and very clearly. It's very useful. RW1:368

Notice how Carnie apparently has skills that Wingate finds useful. Who are these people who "just don't get it" (28.4)? If they are other scientists, then surely Carnie has found a privileged position. For Carnie is understood by Wingate to have as an artistic theme an interest in migration and travel. As we shall see, those concepts formed part of their discussion. And questions of cell movement and orientation – the topographical concerns of map makers – formed part of the common ground between Carnie and Wingate.

How detailed is Wingate prepared to be? Apparently this visual knowledge of Carnie extends to dealing with the behaviour of cells:

> 29 I think Andrew actually came up with one or two good ideas, good points, I'd go away thinking, yeah that's a good aspect I hadn't thought about - because – as you started out asking about what I do, a lot of boundaries, stop points, start points, reasons for doing things, are the language that Andrew is dealing with in art. And so if I've missed out something, like "oh well this cell goes this way, round here, turns right, turns left and he says "Just a second, didn't you just say it does a U-turn there, why does it do that?" – he spots something, there's nothing to stop him from spotting something that I would have missed.
> RW1:351

I noted that Hartley was both a sculptor and someone interested in issues of face deformation. Carnie is both a photographic artist and someone interested in issues of migration and travel. We might ask sceptically: is there any role for these conceptual interests in mediating the integration of the artists' practical skills with those of the scientists? Again, there is a fallacy lurking here, because the scepticism depends on the separation of the artist's practical skills from his conceptual interests.

In both my interviews with Wingate and with Thompson I explored how the artists' conceptual interests shaped the work when artist and scientist found themselves working together on an apparently technical problem. The answers covered a range of possibilities. In the following quotes we can see a move between practical necessity, the role of language, and, hesitantly, the reflection that the artist's interest in migration is having some, unspecified, impact.

> 30 ... so we are interested in every aspect of why they are patterned to move; what they sense when they are moving, to give them an idea of where they are going, lots of chemical gradients, how they actually physically translocate, and how does the internal skeleton of a single cell reorganize itself, pull the contents of the cell from A to B – it's completely unknown – what they do at the other end, and in particular at the moment, why they stop, how do they realize that they have reached a destination. RW1:79

Yet if these ideas can be derived from Wingate's words, they formed no explicit belief, as shown by this quote, taken from the same part of the interview.

> 31 ... going back to your original question about migration – I don't think the themes are particularly useful, because they are emotional themes and scientifically it doesn't mean anything, it just doesn't mean anything. The actual technical approach and the way we see .... It's more the way we see. That's what it comes down to. RW1:533

Admittedly:

32 ...there's more to it than coincidence; his thoughts about migration, movement, meeting, death, experience, life experience, reflect on the way we think of cells experience, birth, movement; its quite interesting. RW2:66

The implication is that Carnie's interest in migration is motivating his interest in Wingate's neurobiological research. The scientist, in an unusual sentence, has run together the artist's research programme "...migration, meeting, death, experience..." (32.1), with the work the scientist is pursuing, work whose metaphorical language seems remarkably familiar to the artist "...cells experience, birth, movement;..." (32.3). The scientist himself is struck by the similarity, and finds it significant (32.1)

d) Alf Linney

A different example comes from Professor Alf Linney, who has undertaken many collaborations, especially with digital artists and photographers. Here again there is some kind of technical congruence between their work (perhaps this is always necessary). Linney however spoke of a sort of mismatch of expectation being common, and important:

33 All the artists I've worked with have wanted to do things beyond what at that stage I thought was possible. I guess that's the way things are, they have a vision of something, which when you try to put it into practice becomes a very difficult task from the scientific point of view. Often the means of doing the particular project don't exist at the time you start, and there's a bit of a gamble. AL:42

One can imagine the artist having only a faint idea of what is technically possible. In fact Alexa Wright, an accomplished photographer, produces work where images of the face or body directly confront the viewer with their own preconceptions of what a face or body should look like. The challenge can be made even more direct if, through digital technology, the viewer sees their own face, and watches it morph.

> 34 I feel in a sense that's what I like about it as well, that I've got a bit of an opportunity of pushing the frontiers of something a bit, in order to achieve something with a bit of momentum behind it. If you've got somebody with a strong desire to do something, and you have the possibility perhaps of making it happen then that provides you with a very good motive. AL:46

Again we see the artist being invited in: a tacit agreement that their expectations might be valuable. The artistic vision becomes part of the scientific question. This doesn't fit well with the idea of art taking its place in early conversations that are generative; it sounds more like artistic practice taking an organising role within an iterative process that moves towards a mutually-agreed goal. In this sense the artist has power, and pushes the project on.

Part of the motive lies in an issue that is more important to a working artist than it is to a scientist – the importance of exhibiting the work. An exhibition sets demands over deadlines, catalogue publication and publicity, matters less common to scientific productivity.

> 35 There is an injection of enthusiasm which actually pushes the thing forward perhaps faster than it would have gone. You've got a goal, to generate some object that is going to be exhibited somewhere, and that obviously does focus

everyone. An enthusiasm to see it out on public display. So for a scientist it is very good from that point of view. AL:69

e) Philip Kilner

My fourth example of artistic practice having an impact on science, in this case on clinical diagnosis, comes from the cardiologist and medical imager, Philip Kilner, who works at the Royal Brompton Hospital, London. It is an important part of the story that, straight after his medical training in the 1970s, Kilner turned away from medicine, and had an interlude of several years in a completely different environment. He worked for a while in a community for the disabled; spent a year in a cottage in Ireland; finally he went to the Steiner-based art college, Emerson College (East Sussex, UK), to train as a sculptor. It turns out that this second training, coming at the end of some years of *Wanderjahr*, gave Kilner the means to return to medicine.

36 As a child I was interested and encouraged in drawing and painting, and it was something I was reasonably good at. And then I went to a school that quite favoured sciences, and particularly towards medicine. And I fell into that stream, and wasn't allowed time to do an A level in Art, which I would have liked to have done; but nevertheless kept up my interest in arts, but had the experience then I suppose, and certainly through medical school, that the two interests were rather separate. PK:8

It is a classic account of separation and loss: a forced inability to pursue an interest.

37 ... it was rather frustrating that an important part of me was being a visual artist, and that didn't get expressed in medical studies. It wasn't recognised, and the whole approach to medical studies seemed so analytical, in a sense fragmented, which wasn't - which went against my nature in a way. So that was a frustration, and one of the reasons why later I chose to leave medicine...

PK:14

At Emerson College Kilner's teacher was John Wilkes. Together they worked on a form of water oscillator, essentially a device where water input is constant, but the water efflux oscillates – an effect achieved not by any valve system but by the form of the sculptured surface over which the water flows. It was this work that inspired Kilner back into the medical world, in particular cardiology. Nowadays he sits in an office in the Royal Brompton Hospital scrutinising scans of patients' hearts. Kilner asserts that his particular style of work, and its success links back to his arts in ways related to practice, and to theory. The practice concerns Kilner's experience, as a sculptor, of dealing with fluids with his hands; the theoretical understanding is due to Goethe, whose interest in form, and interest in the concept of the scientific method, are profoundly important at Emerson College.

I asked Kilner how this return to medicine took place.

38 That's interesting. I didn't quite know what, but I was very happy teaching, and there then did come a point where I began to have a family and could do with a proper salary again. Interestingly it coincided with a point where I had done quite a lot of flow experiments literally at my kitchen sink, to do with cavities about the size of heart cavities, and elastic materials as well as solid

materials, looking at oscillations and flow patterns; I included wine glasses with swirls in, looking at little rocking cavities with asymmetrical flows, and I put together a video tape because I really thought there were some discoveries in here, I really thought there were some discoveries about the heart that – I didn't know if people knew about them or not. PK:224

Kilner investigated some medical centres – "I went there with my little models" – and before long was working under a British Heart Foundation grant developing a variant of a surgical operation for fixing a congenital problem where children are born with only one ventricle (Kilner et al, 1989). We can see here a similarity with the example drawn from the collaboration between Paddy Hartley and Ian Thompson: the craft skills he developed as a sculptor, concerning form and liquid flow, became a new grounding for his expertise in understanding the movement of blood through the heart.

Kilner's artistic expertise involves form, in particular in relation to fluids, but he stresses that complementing this is the theoretical attitude he gained from Goethian philosophy of science. He comments that the fluid dynamics formulae that engineers use to study liquids are of limited value in understanding cardiac flow.

39 [All] those formulae can deal with is really, in many senses, linear structures, linear in terms of incompliant parallel tubes with a constant diameter, and linear in terms of continuous flow. But flow through hearts is not like that in any respect. Nothing is linear. Everything is changing direction, the walls are compliant, and what's more their compliance is non-linear – as they stretch they become tighter, it's not pure elasticity, it's a combination of elasticity and

containing fibres – and what's more they are contractile, and what's more they have compliant valves in there. So nothing there is linear, which makes it actually an insoluble problem in terms - even current computing cannot solve the problem of recreating a heart and its valves. PK:289

The intractable problem of fluid dynamics within the heart is partly alleviated by his artistic training; according to Kilner, he complements his technical skill in scanning technology with an artistic skill[92] in interpretation.

Kilner's early rejection of medicine – and his statements about his artistic interests at school – are significant in helping us understand the role Kilner now gives to his arts background as a component of his medical practice. It is clear that his artistic experience is in constant use. His art is bringing something to a scientific practice that has taken a certain direction, that to his mind has lost something, and that fails to express something Kilner considers important.

I asked Kilner whether he could explain how this style of his might influence his medical practice.

40 The science-art I suppose I do all the time when I look at patients' images, which is a lot of what I do. I work particularly with congenital heart disease, which involves form abnormalities and flow abnormalities of the heart, so when I am looking at 2-dimensional moving images, in particular slices, you are imagining the 3rd dimension, and how they relate to each other; and my

[92] Kilner used the term 'intuition'.

colleagues know that I'm particularly good at selecting image planes, or interpreting an image plane into a 3-dimensional structure. They don't know why, they just think its something I'm not bad at. PK:377

This interesting quote is more than a bald analysis of technical skills. He makes the link between his looking at images, and the art – and explains why (the selection of image planes). Yet apart from the explicit reference to "science-art" (40.1) there are many references which, against the background knowledge of Kilner's other training, become significant. The bald fact of "looking at 2-dimensional images" (40.4) is transformed into something more challenging: "imagining the 3rd dimension" (40.5). The use of the term *imagination* is significant; and we can note the references to colleagues who see his expertise (40.6), but can't quite work out why he has this skill (40.8). The implication is that they don't understand completely the source of his expertise, because it is a craft skill rooted not only in medical knowledge and experience, but in sculptural practice and Goethian philosophy.

Excerpt 40 has important echoes of some comments we made about Richard Wingate and Andrew Carnie. Kilner is looking at 2-dimensional 'slices', and imagining the 3-D form. This is precisely analogous to the Wingate/Carnie enterprise. There we saw that the scientist and the artist together worked on 2-D images of neurones, finding the 3-D form (its extension in space); further, they considered how slices could relate to the movement of cells in time and space. In short, they considered form, and how form changes. Understanding form is also of course the central preoccupation of my other pairing in this section, Ian Thompson and Paddy

Hartley[93].

I asked Kilner about his interest in Goethe. How could the 18th century writer influence a contemporary clinician?

41 I think another aspect of Goethe as a figure is his power of imagination. And what I would say about this is it's informed imagination, it's not free-flight imagination. He's always coming back to observations, his imagination is informed and tutored by what he's observed. And you can do that in fluid dynamics, and then you can say bring your previous experience to cardiac imaging now, which is what I'm in. With imaging you see phenomena which you just recognise from having done flow experiments in vitro – you just know that that's a vortex in there, or that's a line of shear, or turbulence. PK:305

42 ... and the other thing is you can extend your imagination through the circulation as a whole, not just looking at flows through a heart cavity, but you know that that connects upstream with the veins, and downstream with the arteries and you can look in textbooks for information about flow velocities in arteries and their branches, the numbers of branches, the size of branches, you can begin to use your imagination to build up a kind of fluid imagination of

[93] A link between Kilner, and Thompson/Hartley is that while all of them are relating form to function, they are also profoundly aware of, and struggling intimately with, the relationship between their chosen material (glass, liquid), and form.

what the circulatory system is like, which I find absolutely fascinating, awe-inspiring when you begin to do this. PK:314

43 I think nobody tries to do that. Nobody tries to do that. But you can train your imagination, all the time reined back by actual data and observations, you don't let it fly off, but nevertheless... and you can draw information from many sources, and feed it back into your imagination. Your imagination is a tool, or faculty; it is a genuine scientific faculty, I think. PK320

f) Some background concepts

Although a good proportion of each interview was devoted to the actual work the scientist was doing, both in his normal daily life, and in his collaboration, significant time was devoted to exploring the scientist's wider intellectual positions on the workings of science. This section explores the role of the artist in the development of these positions. Firstly I consider ideas about the nature of evidence; next I turn to attitudes about reductionism.

i) Attitude to 'evidence'

I have suggested that in these conversations the interviewees routinely alternated two, possibly contradictory views. In the transcripts declarations of certainty that their science is insulated from their work with artists are found adjacent to quite elaborate investigations into plainly epistemological issues raised by such work. This section explores the nature of this conflict. In particular I want to raise the following question. If the scientists commonly report that their epistemology underwent some shift through engagement with an artist, is this not also a shift in the way the scientists practice their science? A possible clue as to why scientists might not consider their testimony inconsistent is shown by the quotes in Section I, for example excerpt 8. Here, the scientific method has been tooled-down to something extraordinarily simple and remote: it isn't 'really' part of laboratory life (literally so: the example is set in the tropical rain forest).

In the following, Richard Wingate investigates what he calls the fragility or partiality of science. In a section of the interview where I asked about the way Wingate talked to Carnie, and in particular what intellectual arena they shared, the work of Ramon y Cajal, and of Eadweard Muybridge figured prominently. Cajal developed a method of staining that made the brain visible by staining only a small selection of cells, and is a figure from the history of science admired, and written about, by both Wingate and Carnie. Eadweard Muybridge, an important source for Carnie, is best known for his freeze-frame photography of people and animals in motion.

Wingate developed in the interview a series of ideas about partiality, or fragility. That is, science depends upon observations that are never comprehensive. In Wingate's case, the intellectual basis for this comes not from a philosopher, but from Cajal. If every neurone stained, nothing of the brain could be seen. It is a good example of understanding depending on selection, on not seeing everything.

Here are four excerpts, each of which addresses the Wingate/Carnie project *Magic Forest.*

44 [the brain] cannot be visualised, except in pieces. RW1:299

45 You take representative individuals...put them together to make a sequence... slice up this tissue and take one in a hundred slices ... RW1:302

Remember that the installation *Magic Forest* shows the movement and development of brain neurones, as images light up, and disappear, on three muslin screens. Seen most simply, the installation seems to be an attractive 3-dimensional representation of fluorescing brain neurones – an aestheticisation of neurology. At another level, the work invites thought about the methods of science, in particular the historic dependence of brain neurology on the microtome. Sections are taken, the brain is sliced, and a narrative constructed from individual samples.

46 It [*Magic Forest]* evokes the issue of slicing, time slicing and real slicing, and has something of the methodology in it as well as showing something of the process. It's quite, I think it's almost an experiment on Andrew's part, and I think he's pleased with the way it works out; but having those components in it made me think about what I was doing ... RW1:310

47 ... probably working with Andrew ... has made me think, particularly, about the partialness of it, that we are getting partial pictures. And putting that back that back together again ... that is the end goal ... This is something I've been thinking about a bit with Andrew, or independently from Andrew. I can't say it's directly resulted from conversations with Andrew, but I think it's been triggered by that. RW:410

48 I think his contribution has made me pause – I was always interested – but more actively, is this something that people should be more aware of, the kind of frailties of ... yeah the frailties of what we are doing RW1:503

Here, Wingate repeatedly links his new understanding of the nature of evidence with working with Carnie. It is a relationship where the artist's influence can hardly be doubted. The artist "made me think" (47.1), "made me pause" (48.1). Wingate remains reluctant to credit Carnie as directly responsible for his new metaphysical reflections, but suggests instead the metaphor of the trigger (47.6).

Previously I quoted Wingate's descriptions of sitting down with Carnie and looking at images of neurones. Those quotes, I suggested, are plausible accounts of artistic practice intervening in scientific practice. But might not Carnie, who has a degree in zoology, be better seen in this context as having been re-configured: he has become a technician?

I considered this point in relation to the artist Paddy Hartley (excerpts 26 and 27). In the interview with Thompson I was not able easily to fill out a picture that showed the full scope of resources Thompson took from Hartley. At first sight perhaps those resources were limited to Hartley's skill in casting. There are however oblique hints that allow us to infer a fuller role for Hartley that Thompson did not in fact articulate.

With Wingate and Carnie the picture is far clearer. For the art product *Magic Forest* is a sharp reminder that the continuities of neuronal development are built from partial pictures. It was a feature of the interview with Wingate, and the unrecorded 'pilot' interview with Carnie, that the two men spent a great deal of time, at a lab bench, looking at images and drawing them out, and talking about what they saw. Those conversations must be considered as important a product of the collaboration as the installation *Magic Forest.* Excerpts 46-48 imply that the time spent on those conversations, held in the Kings College lab in between Wingate's duties and experiments, using time pinched out from his time on duties and experiment schedules, had their effect on Wingate's scientific thinking. We have earlier referred to ideas that describe the severe discipline of laboratory life as entraining all who come close, with the possibility of science extending its scope outside the laboratory as dependent on a 're-ordering' of that outside. With this example we can put forward another, symmetrical proposition – that in some cases, the artist coming into the orbit of the scientist effects a change himself. For here we have Carnie's own artistic interest in partialness, and his thinking behind *Magic Forest,* being taken by Wingate as impacting on his own understanding of the nature of evidence.

First, "working with Andrew"(47.1), is linked in Wingate's narrative with a reflection on the scientist's work. Such work is "partial"; in the case of Wingate, he becomes more acutely aware of the way the images he works with are tiny fragments of the whole, and must "be put back together again" (47.3). We can take "the work with Andrew" to refer to those table-top discussions, in the lab, of images of cells; or to the installation in the Science Museum. Wingate on the one hand signals the role of the artist in this (he was the "trigger" 47.6); but on the other hand, he nuances this declaration by vetoing talk of simple influence ("I can't say it's directly resulted from conversations with Andrew...) (47.5).

Once again, the discourse is unstable. There is a tension about the best way to describe the causal route of Carnie's influence that reminds us of the work of Henderson encountered in the literary review. These comments make plain that even with such a successful collaboration, with rich consequences readily apparent, it is difficult for scientists to find a simple discourse that captures adequately the laboratory relevance of the artist's presence.

Manifested as lights in a darkened gallery, but worked up originally through hours of work and conversation in a Kings College lab, the installation is close enough to scientific process to gain Wingate's respect, but is also transformative (46.3). It is an experiment, but one performed by Carnie.

To conclude, Wingate sees links between his artistic encounter and his own reflections on the vulnerability of his method. Perhaps he would have thought along these lines anyway, but Carnie, and *Magic Forest*, induced some more articulate elaboration. During his second interview, Wingate made a connection between the work with Carnie and his response to an invitation from Nature Reviews Neurobiology to write a 'Perspectives' article on the history of his subject. At this time the article was making little headway but its themes were clearly going to draw from some of the issues discussed with Carnie. Ultimately the article was published in 2006, co-authored by an art historian at Oxford University based in Martin Kemp's unit (Wingate and Kwimt 2006).

Wingate's interest was in an epistemological problem, namely the 'weakness' of scientific evidence. The theme of weakness or frailty was often present in the way the scientists talked about their collaborations. For example we shall see in the interview with Anthony Bull that one of his concerns was the institutional frailty of artists. We saw too that Alf Linney imagined that some scientists might see science as in some way 'vulnerable' to the attentions of artists, as it had been to the inclinations of Stalin, or the Vatican.

In the following excerpt, a slip-of-the-tongue takes the psychologist Mike Page into a long discourse on the creative significance of vulnerability:

49 I think in this creative step maybe they have their, maybe they can perhaps understand each other best by sitting there thinking "what next" and requiring some sort of recombination that they both require and perhaps if they could teach each other how to attain that sort of nirvana of generation, the generative step, then I think that is probably where the science/art collaboration is vulnerable. But apart from that I mean, it, it.... Vulnerable? MP:138

50 SW Valuable!

MP Valuable!

SW Freud

Freud is coming in, no I think it's tiredness there........is valuable (*pause*) maybe vulnerability does come into it, maybe scientists do need to make themselves vulnerable in the way that artists make themselves vulnerable every day – interesting slip! – I mean artists make themselves vulnerable because they, their work is assessed by a public whose opinions really decide their fate whereas a scientist can at least appeal to the data and say "look I am right". MP:151

51 An artist has no recourse to that so makes themselves vulnerable by saying well look this is what I think maybe you like it maybe you don't that is the very vulnerable position that the artist is in. If the scientist has done their work well and has come up with a generally good hypothesis (and that assumes the generative step), then they can literally almost prove that they have a better idea than someone else so they don't need to rely on luck or whim or opinion or whatever. MP:156

52 ...so the artist is in a more vulnerable position, maybe the scientist in order to think has to put themselves in a vulnerable position but of course what they need in order to do that, like artists I suppose, is a fairly stable institutional background that enables, gives them the freedom to make mistakes and to think outside of the box, probably that's extended more generously to scientists than it is to artists though less, increasingly less so I would say... MP:162

53 ...increasingly more that you have to have all of your ideas tied up before anyone will give you any money rather than someone giving you the money and you saying then I'll come up with some ideas, give me a year to think about it, no-one is going to do it these days. Whereas I suspect Crick and Watson did nothing else. MP:167

These excerpts are notable for the way they inadvertently initiate an enquiry into the interesting dynamic between security and vulnerability within the creative process. The sequence starts with the interviewer spotting the slip-of-the-tongue, playing it back to Page with some implicit psychoanalytic theorising: "Freud". Page, a cognitive psychologist, dismisses the link, allows that tiredness may be a factor in his miswording, and then decides to explore further the strange relation between 'vulnerable' and 'valuable'. His key method here is to explore the relation by noting certain aspects of the different professions of artists and scientists. Most interesting is the way a paradox is placed at the heart of the matter. Scientists are invulnerable because of their scientific method; with a good hypothesis (51.4), and data (50.7) they "...don't need to rely on luck or whim or whatever..." (51.7). Moreover, along with the securities of the scientific process, scientists benefit from a stable institutional background. The difficulty that Page alerts us to is that the very thing that might allow one to think outside the box (52.5), namely the steady regularities of the scientific day, can turn into something quite different – a straightjacket. These days the institutions set too many demands and constraints. The reference to Crick and Watson, biology's paradigm case of brilliance and creativity, underscores what we might be in danger of losing.

Hence the necessity for scientists to associate with artists. When the management structure stifles creativity, you cannot 'think' outside the box unless you 'look' outside the box. Artists are vulnerable epistemologically because their work and their fate is judged by the public. They are vulnerable too because they have no institutional security. But meanwhile they have the freedom the scientists are beginning to lose, and presumably beginning to miss.

ii. Attitude to reductionism

Earlier, in the literature review, and in my methodology chapter, I raised on several occasions the way my study might relate to debates about reductionism. In the literature I studied, Kemp was the most obvious example of a link being made between reductionism and art (Kemp 1999).

Reductionism should be understood as the programme devoted to finding explanation for natural phenomena in terms of components. Reductionism has been a compelling issue for traditional philosophers of science, because of its ontological aspects. This is not the place to exhaustively discuss the philosophical and scientific implications of reductionism. The important point is that we should distinguish between overtly philosophical debates about reductionism, and debates that are more cultural in tenor, and involve the way we organise ourselves into specialisms, and pursue a career by staking out a particular and tiny territory.[94] In terms of philosophy it is enough to say that reductionism implies physicalism (phenomena are to be understood in terms of physics), and monism (in principle all scientific explanation is united around a single set of principles). In fact this distinction between 'philosophical reductionism' and 'pragmatic reductionism' (Webster 2003) is forcefully present in the following quotes. Reductionism is a controversial topic because it accretes around it ideas about the priorities science should make in ordering nature. Reductionism, in other words, can be seen as ideological, a series of statements about the best way to understand nature. For some, reductionist thinking is the cardinal epistemic virtue. For others, there is another epistemic virtue, an interest in 'systems thinking', of vital necessity in compensating for biology's molecular gaze. In the excerpts below we see the scientists not only looking for a balance between the two, but enrolling artistic practice as an important aspect of achieving that balance.

[94] This interpretation is said to be particularly applicable to biology, with its move into biochemistry and molecular genetics.

When a scientist rather than a philosopher worries about reductionism, the concern is that a components-led approach impoverishes the scientist's engagement with the wider field, and leads to a proliferation of disconnected theory. Reductionist thinking deploys the austere rigidity of the laboratory towards ever finer manipulation of the very small, and thus makes it more and more difficult for the scientist to relate his work to various 'high level' fields such as organismic biology, systems biology and ecology. The interviews confirm ethnomethodological studies of laboratories as worlds of exceptional busyness, prioritising, and artifice. While it is possible to imagine molecular biologists taking the time to read journals outside their field, button-hole unknown colleagues in the corridor, and venture off to alien conferences, Mark Miodownik suggests that this sort of thing is rare.

54 MM ...basically, it's not that usual for people to pitch up on your doorstep.

SW Does it seem usual for scientists to do that?

MM No, it's unusual. MM1:159

Miodownik implies that if broadness of mind is a scientific virtue, it is one rarely practiced. And when a virtue is neither sought nor practiced, it vanishes.

From the quotes below, it would appear that scientists might treasure their contact with artists as a good way of re-discovering this virtue, articulating and 'stabilising' it as part of the scientists' intellectual and institutional repertoire.

We should not conclude from this discussion that artists somehow introduce scientists to a debate about the relative merits of reductionism and non-reductionism. The way the topic rose so easily within the interview, and provoked so rich an exchange, suggests the issue is already a live one for this particular constituency. We can see however the scientists making a serious effort to relate their artistic encounter to a pre-existing, often fraught, attitude to reductionism. I infer this fraughtness from the way the scientists very often describe their struggle to find an inclusive vision as very distinct from, and even antipathetic to, the inclinations of their colleagues. The conclusion I draw is that the art helps these scientists articulate a non-reductionist vision. This vision, it turns out, is not simply a background philosophy. In most of these cases, I suggest, the vision, constructed or supported through an artistic encounter, becomes manifest in professional practice.

These 6 excerpts, from Nick Davey, Richard Wingate and Alf Linney explore the issues.

55 Yeah, you might argue then that one day we'll be able to make a computer do what the brain does, but I don't think you'll ever have that spirituality in whatever it is, even if you measure , they have these machines that measure posture and movement, you know you put detectors all over the body and even if you measure it with that, it might look exactly the same as a real human being, but it wouldn't have that extra presence. ND:540

56 It's rather like programming a pianola to play a piece of music, and a pianist playing – there's something extra, and that I don't think science will ever explain. ND:545

57 The other thing that is probably similar to Andrew, or to Andrew's artistic approach, is also looking at structure independently of its molecular or cellular... well look, this is a structure, it has a certain logic to it, a form, you could predict something from that form, it is not just there accidentally. RW2:149

58 I probably have changed my view just putting it in context may have been a product from talking to Andrew and everything that's followed on from that. It's definitely I feel recently that I've been able to put these element of what I do clearly in different positions: I'm going to do some science here, some exploration here, I'm going to think about the global picture here, and the context. RW2:340

59 That's what artists often do for me, gluing together lots of different things that in science would be in separate boxes... We tend to split things and split things until you get down to atoms and even smaller, and there out there is a whole universe, and structures that belong to a different level of detail and interest, and that is probably what the artist comes at with you. AL:210

60 Obviously the artist is going to introduce all sorts of things like personality, and psyche into the discussion, well I say obviously, most of them do, and then the scientist is a bit lost, because they are working at the other end of the scale of things, but it does certainly force the way you think along different lines, you are forced to contemplate things in a different way... AL:237

61 the artist wanting to think of the human being as not simply as something generated by a whole load of genes or the genome; has something that exists at a different level. AL:220

In these series of quotes we see a remarkably clear account of the various issues concerning reductionism. In excerpts 55 and 56 we see the classic exposition that science has limits to what it can express. As is typical, the region beyond those limits is hard to describe: it is, literally, 'extra' (55.7; 56.2). The agreement to limit science's ambitions is controversial especially within Nick Davey's field, neurophysiology, and there are well known opposing views constantly expressed in the trade press and in popular culture (Greenfield 2000; Kandel 2006; Lythgoe 2007). In this respect Davey's view, though not innovative, suggests an active sensibility of the problem.

Note that excerpts 57 and 58 put the artist fully into relation with the scientist's interest in the non-reductive vision. Carnie's artistic approach (57.1) has a similarity with Wingate's non-reductionist interest in structure and form (57.3). The talking – the spending time with the artist – may even be credited with changing a viewpoint in science (58.1). After talking to the artist, the global picture and the context begin to push in on laboratory life (58.5).

Finally, Alf Linney adds a dynamic aspect, situating the artist as a force in a struggle these alert scientists play out each day. In excerpts 59 and 60 the picture is of the scientist habituated to just a few rungs on a long 'ontological ladder' that extends downwards towards quarks, and upwards towards ecosystems and beyond. Artists make you aware of where you are on that ladder, and remind you of the upper reaches. Artists are interested in the psyche and personality, and so the scientist is required to "contemplate things in a different way" (60.5).

62 Maybe ultimately you can analyse all this and connect it all together. But at the moment of course we can't. We don't have sufficient knowledge to connect all these things, we don't even know how things take on their form, how the genome gets translated into a form, let alone into a spirit, or a person. So you know those sorts of issues are particularly impressed on scientists by artists.... AL:223

It is worth lingering on Linney's suggestion about being encouraged to think in a certain way (60.6). He in fact uses the words "force" and "impressed on". We are invited to reflect that what is under discussion is not recreational intellectualism, where the distinguished professor gently ponders on the meaning of life as he commutes back to the suburbs. Linney suggests instead that this is a struggle going on in the workplace, relevant to priorities being decided, and to relationships with colleagues.

This idea is strikingly expressed in the following set of excerpts. Notice the way the scientists, having articulated their philosophy, are able to set themselves slightly apart from their colleagues (64.1, 65.6, 67.5). Thus is implied that these scientists may be involved in (at least) two struggles in their professional life. Firstly there is an intellectual struggle to position their practice in a manner appropriate to the richness of the discipline. Secondly, the construction of their own identity – their own autonomy – within the group is a constant issue.

63 Reductionism is very prevalent in this particular centre, it reflects the current trend toward molecular neuroscience and molecular science, and this thing called bioinformatics, where you pick on a certain molecule – it could be a gene, it could be the protein product of the gene, a particular thing – and then you see how many processes it's involved with, and your focus is on the gene; that can lead you anywhere, but the reductionist approach really is looking at a series, ends up being a series of biochemical pathways, in an abstract sense, not related to the brain. RW1:115

64 So why am I unusual? I'm trying to integrate the two things together, and look at the whole hierarchy of processes, maybe from the internal skeleton of a single cell right through to the relationships between multiple populations of cells and how they interact. That's not impossible by any means, but its more putting the actual work- which is probably very similar to everyone else's – into a context which is larger. I wouldn't claim to be superior in any way, but I think it gives certain advantages, definitely. RW1:121

65 You can see it here; this place is now auditory research. There are so many people devoted to tiny little fragments of it. Microns in size. Even understanding that is beyond them. And you may forget you are in the ear. What is hearing to do with? Listening to music, rhythms that change the body. You can see the attraction of trying to get out of this little space. They end up with blinkers. AL169

66 SW You mentioned earlier holism. You said there were various ways of looking at the brain. You seemed to suggest that you yourself can do the stuff, you had a holistic approach; you saw yourself as quite distinct, that you were quite inclined to take a whole view of the brain. Is that correct? RW2:128

RW Yes. Distinct, it almost smacks of ego. Yeah I'd say that is a distinct approach. We can encapsulate it in science by saying oh, he's a systems guy but I'm still... I know I've got a slightly different view from other people ... I actually think this way for a reason, because I think its a successful way of looking at systems, and I think it is slightly distinct, because of the way science has gone, and it has followed a technical course, not through the imaging but largely through the molecular biology, and the development of things like polymerase chain reaction... RW2:133

67 It's interesting that .... I don't think many people look ahead more than .... I know quite a few scientists who find it difficult to look ahead beyond the next small experiment, and actually get quite nervous about long term prospects. Pretty much the most successful scientists in my field are the ones who've had a broader view, particularly technically. RW1:405

Later in this chapter I discuss the interview with Jane Prophet, and how her interest in complexity theory apparently impacted on Neil Theise's theorizing on stem cells. It is an example of an artist's thinking having an effect on a scientist's own theorizing. Another artist I interviewed, Marilène Oliver, was also explicit in her utilization of non-reductionist thinking as a corrective to – or a reminder of - current trends in scanning technology. She writes that her work re-asserts the physicality of the human body not by challenging the molecular orientation of biomedicine, but rather by questioning its new embrace of medical imaging in clinical diagnosis: "New technologies, especially communications and medical imaging alienate us from the bodies that we have".[95] The familiarity of this style of image, and its power as a main source of truth about the health and frailties of our bodies, is disturbing to Oliver. She therefore devotes much of her work to re-building whole bodies from medical images.

There are interesting complexities here. For imaging technology in medicine has tended to be viewed not as reductionist: it principally reveals structures rather than molecules. They may usually be two-dimensional, but they are often life-size. In three dimensions, or as animation with a time line, they are even more expressive of the entire entity of the human organ.

Just as I was concerned about the danger of over-simplifying the artistic commitment to a non-reductionist vision, so I felt also that the rise of 'systems biology' in academic life sciences research might suggest that, institutionally, reductionism was already on the decline. I wanted to explore the sociology of reductionism more fully with Richard Wingate.

Firstly he confirmed that the publication and career system makes reductionism a safe strategy.

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[95] www.marilene.co.uk/. Accessed November 4, 2006.

68 If you were to look at publications, then yes it's component-oriented, it's molecular. And there's still a recipe for a successful publication in my field which is identify a molecule for the first time, see where it works, interfere with its function, show the system collapses, put it back again in some way, and show the system recovers. That's the recipe. RW2:218

69 [and then there is] the bulk of scientists in the field who have been trained and come through and built their labs on the back of a reductionist approach: take a molecule, look at it, move on to another molecule, look at it, move on. RW2:236

70 You start as reductionist because you have to. I've been lucky in the sense that I havn't had to do that. I've published one paper which has been a description of a gene and I avoid it and I'm fortunate; most people here where I work have to spin out one paper after the other saying, 'Characterisation of gene elph 16 and its expression in the chick.' RW2:259

But what about 'systems' biology? Wasn't this having an effect on the way projects are conceived? According to Wingate, that would probably be a misinterpretation. Only a few scientists, he implies, can think 'holistically'. His suggestion was that in the grand theorising of systems biology (such as in metabolomics) only those at the top of the profession were in a position to 'make the connections' that are necessary to higher-level explanation. For this work, they rely on a steady stream of gene characterisations and 'knock out' protocols, carried out systematically by teams of junior staff (excerpt 70). Wingate described a lab he knew in the United States, controlled by a prestigious and powerful systems biologist.

71 ...he's got a hugely productive factory lab producing great scientists...By employing this team of reductionists he is sitting on top – this is my view of him – he is collating this and he has got the overall approach, but the power is at the sharp end of molecular biology, so the key has been harnessing that, which is why we see this trend towards factory labs. You can have twenty or thirty people, and it's a bit of an odd social structure, because you do tend to have the lab head and then a whole slew of maybe 30 acolytes who battle it out to become the next lab head. It's kind of feudal... there's only a few people who are in a position to think holistically. RW2:241

Wingate describes a competitive system where the main task of most people is to excavate the details of a system, rather than understand it overall. That task is for only a few individuals, namely those established at the top of the hierarchy. It is easy to see that such people, making the running in the University publicity machines, and doing the most prestigious publishing, would give the impression that molecular biology has found some new balance after decades of reductionist prioritising. Wingate suggests that for the majority of jobbing biologists, that vision remains remote.

Sometimes scientists linked reductionism with something apparently threatening to normal conceptions of science, namely its utility. Here, Mark Miodownik finds his caution in commenting on another specialism a subject of criticism.

72 I remember very clearly giving a talk going to a school when I was at Oxford doing my PhD and I'd be slung out to schools to talk about science. I was in the common room .... this almost holy ... and the science teacher said to me, oh I've got this other question (about this that and the other) and I just said, I don't know that's not my area, and she just had a fit, completely had a fit at me, and she said: "you scientists are all like that, you say its not your area, and you wash your hands of it, and it's really bad" . MM1:365

Miodownik was disturbed by the experience. He was struck by the way the teacher interpreted the detachment of the scientist as hostile, a form of passive aggression. He suggested that an unthinking embrace of specialisation can end with you becoming "sort of useless".

Might the science training and career structure encourage, so to speak, "becoming useless"? Davey complained that PhD programmes were:

73 ...producing huge numbers of stamp-collecting molecular biologists, with no – this is being horrible – initiative, no sort of questioning of the world around them. They are doing it because they are told to do it, and because they know how to do it, and because the person working in the lab with them does it in the same way. ND:725

Perhaps this is the thrust of the following statement.

74 Science has become more compartmentalised. May be that would militate against scientists engaging in art, but maybe that is a safety valve in a sense for this narrowing, this moving into a narrower and narrower path. There is a danger, and most scientists would recognise this, of tunnelling down into a tiny channel. AL:266

We should remember that these scientists are not condemning reductionism. All of them are successful exponents of contemporary science, and work at very tiny scales. In the section of the interviews where the topic of reductionism was encountered, they worked hard to suggest that the richest value was in finding some kind of balance.

75 I do think it [the artistic perspective] has given me a broader perspective; in a sense it has reinforced my feelings about how – its sort of the holistic approach if you like – about thinking about the thing in the whole, rather than being too reductionist. By the nature of what we do, it is reductionist in its approach. But I don't want to feel categorized as being a reductionist as being the whole answer. I guess I try to see the broader context of everything. TH:246

In fact, Tony Holder found 'his' artist too committed to ignoring the molecular aspects of the work.

76 What I, and I don't know whether this goes to the question as well, what I found was that, what was unsatisfactory about the relationship, amongst other things, was that I could never somehow get her interested in things that were

submicroscopic. A lot of things I'm interested in, whether its the parasite, or the molecules, or whatever, the sort of things where you need to enhance the senses to be able to access them, whether its by microscopy or modeling or whatever, at the end of the day the only thing she was interested in was mosquitos, and for me that was only part of the whole thing. TH:364

This has been an extended discussion of reductionism, reflecting the way the topic took up substantial time in the interviews, and represented an important route for the scientists to explore the role of artistic practice in their work. It is plain that the word reductionism evokes a great many issues, some philosophical, some more practical. It was noteworthy, I argued, that these scientists often suggested a connection with self-image: their autonomy within both the institution and the discipline depended it seemed on some active appreciation of the opportunities and the perils of the reductionist enterprise. In all of this, the most important aspect for the current study is the consistent and diverse manner in which the scientists use their relation with artistic practice to help explore the problem, and perhaps take up a new position.

The excerpts, I suggest, invite the reflection that comments on reductionism, and comments on the art-science collaboration, are eliding. It is not necessary to conclude that the experience of dealing with artistic practice has initiated wholly new thinking in the scientist. The point is more that the collaboration has allowed thinking about reductionism to find its space. These scientists are using their experience of the collaboration to enrich and enliven their position on reductionism.

g) Concluding comments

In discussing their own scientific practice, the scientists point to shifts in perspective they are able to relate to the work with artists. We might ask: are these changes permanent? Is a more 'intellectual attitude', for example in relation to reductionism, an enduring product when the scientist takes up tools with an artist? That might be too crude a conclusion. Yet it is impossible to read these excerpts and not be struck by the idea that the scientists are discussing issues of importance in the scientific life. When Nick Davey discusses "huge numbers of stamp-collecting molecular biologists" (Excerpt 73) he is probably elaborating a concern elaborated long before he met his artist colleague, Kitsou Dubois. On the other hand, given the subject matter of the interview, we might infer from the vehemence of the comment that this scientist anyway, partly through his experiments with Dubois, could never be accused of being a mere "stamp collector".

Wingate also has asserted his independence from 'factory biology' (excerpt 70), and he has made some kind of a link between that autonomy, and his explorations with Andrew Carnie (excerpt 58). Later in the thesis too, we come across the comment from Mark Lythgoe that he found the 'real Mark Lythgoe' through working with an artist (see Concluding Discussion). These are comments that make it sound as though significant changes are being worked out during these collaborations, through the support of intellectual positions becoming better elaborated, or through new understandings of the self in relation to the institution. It is a point I shall return to in the Discussion chapter.

It is also worth at this point underscoring a theme that emerges from time to time within the thesis: that the art-science collaboration is a ground where a number of intellectual tensions and ambivalences might be worked through. We have already seen that the scientists Richard Wingate and Mark Lythgoe ally themselves with an empiricist position in their declaration of the separability between art and science. At the same time they find it easy to link work with artists to apparently important developments in their intellectual and institutional life. In his own interview, Nick Davey sketched out a bewildering number of projects and collaboration, and voiced scepticism not only about the 'scientific' credentials of his artist collaborator, but about the sense of the life science PhD programme, which 'churns out stamp collectors'. Clearly these scientists are expressing ambivalences and tensions not just between art and science, but *within science itself.* They incline to an empiricist model of science, but they were anxious in interview to articulate their own idiosynratic individuated position within their field. If their tacit philosophy of science is monistic and roughly sketched, their appreciation of the daily scientific struggle is highly textured; and they intellectualise it with great attention given to locality and departmental politics. There is no doubt a tension here; and the art-science collaboration is playing a role in resolving the conflict.

Chapter 6: Analysis of the interviews (B)

I: The scientific self

At first sight, the scientists' comments now coming under scrutiny are simply diverse aspects of varying lives. It might appear there can be no organising principle to put them into order. In fact, I suggest, each of the following groups of comments illustrate ways in which these scientists create a 'scientific self' within their professional world. As they set to describing their art-science collaboration, the scientists gradually construct an image of themselves as active agents finessing and shifting the parameters of their work. Committed and successful scientists, the interviews can be construed as showing the art-science collaboration to be an opportunity for the scientist to explore and even re-define his own professional space. In the analysis that follows this opening discussion, a concern about 'scientific-selfhood' is the unifying feature of comments on the working day, on communication between artist and scientist, on the ability of the scientist to work with an artist, and on the likely impact of such work on a scientist's career. I shall return to, and develop this theme in the last section of this thesis, the Discussion chapter. Clearly though, the interview material of this chapter, with its emphasis on daily and diverse forms of practice, has as theoretical background the sociological and philosophical literature that has focused on the activity of the lab. Something of this literature has already been described. Section II forms a more extended discussion.

II. Philosophical discussion

The discussion so far has focused in particular on the way art-science collaborations have been enacted within the laboratory or clinic. The interest was in scientists interacting with artistic practice, even as they assert the autonomy of the scientific method.

In what follows, the focus is broader. Now we will hear scientists discussing their professional life in a way that includes not only their laboratory, but also their wider scientific identity. On the evidence of the transcripts, the scientific identity takes its form not only from research projects, but also from interactions with institutions, visits to cafés, and days at home. My emphasis now contrasts therefore with what has gone before. For example my previous discussions of methodology have so far discussed texts that make their emphasis the nature of life within the biomedical laboratory (Knorr-Cetina 1999). Though these texts recognise that a laboratory must relate to its 'outside', it turns out that such a relationship is only possible if the 'outside' complies with the structures of laboratory life (Latour 1983).

Joseph Rouse's (1987) interpretation of *The Structure of Scientific Revolutions* saw Kuhn as heralding a new interest in what scientists actually do, with debates about the truth of theories – their status as representations of nature – downplayed. Polanyi's (1973) interest in tacit skills, Ravetz's (1971) emphasis of the craft nature of science, and Hacking's (1983) description of the 'performative' aspect of science, are other works from those years when the standard epistemology of science suffered serious reversals. This interest in practice is also obvious in texts already mentioned in this thesis (Knorr-Cetina 1981, 1999; Latour and Woolgar 1986; Traweek 1988; Pickering 1992 and Buchwald 1995)

Yet these works on practice, it must be admitted, imply an almost exclusive emphasis on processes within the laboratory.[96] Social constructivist writers remain strongly focused on the 'socially-embedded' yet technical issues occurring, or discussed, within the laboratory. In a sense, these texts are reductive. For a wider perspective, one that might prove sympathetic to

[96] Traweek's description of physicists' culture is a possible exception. Her comment "I saw men engage in parenting very rarely" (Traweek 1988,p84) is embedded in a passage explaining how success in physics relies on a compliant family constellation.

our current investigation of artists' practice in the orbit of science, Rouse's examination of the pragmatic and hermeneutic tradition within philosophy of science proves useful.

Rouse is a philosopher of science interested in the continental tradition of hermeneutics, drawing especially from Heidegger, and he looks to Michel Foucault as he develops an understanding of scientific knowledge as something involving power relations. He also wishes to specify in detail the implications of an interest in scientific practice. Finally he combines these preoccupations with prolonged arguments that call for philosophers to lose their interest in the justification of scientific belief, and to seek instead the significance of science in how it is practiced, and how it operates in the wider world. His conclusion is that what is needed is a 'cultural studies of science', a field that would largely discount 'foundational' questions concerning realism and correspondence theories of truth, in favour of accounts of science as a series of local practices and local expertises struggling to cohere together and extend their empirical grasp.

Rouse advances a version of pragmatism. What is important to understand about science is not the foundational status of its beliefs, but the manner in which its various practices cohere, grow or fade: "To understand scientific knowledge, we need instead a positive account of the skills and practical know-how that constructs and stabilizes phenomena and that enables scientists to intervene and to manipulate them in informative ways" (Rouse 1987, p21). In Rouse's work, the hermeneutical task is clearly a liberal one. That is to say, Rouse invites us to 'understand' rather than 'explain' the operations of scientific research. There should be no pre-judging of the scope of science's workings. Strict judgements on the logic of science was the province of epistemology. What is needed now is to move on from epistemology (with its foundational prejudices) to a holistic, pragmatic philosophy of science that extends its gaze to the furthest horizons of human culture. Clearly, if the task of philosophers is less legitimation, and more the seeking of the significance of scientists' acts, one would not pass over the opportunity to make a careful scrutiny of scientists' dealings with artists.

It is important to note that Rouse remains aware of the importance of the empirical project within science. That is to say, however rich our interest in culture, and however confident we are in integrating scientific endeavour with matters drawn from far beyond the laboratory, the theories, the measurements and the conclusions of science remain an important locus of philosophical enquiry. In locating philosophers who have forged such a 'post-empiricism', it is Mary Hesse who is singled out. Rouse praises Hesse's effort for questioning the possibility of objectivity while also setting up a pragmatism that sees successful scientific beliefs as those that increase the community's empirical grasp of a field, and that achieve consensus.

Rouse makes significant use of Richard Rorty's *Science and the Mirror of Nature* (Rorty 1979) noting the way it combines an appreciation for the work of Thomas Kuhn with a radical call for the dismissal of epistemology. As with Rouse, Rorty will replace foundational searches with the hermeneutical method. The job of the philosopher is to enable 'edifying conversations', a style of discourse that continually juxtaposes and explores a myriad of scientific ideas and influences. The aim is not so much to seek out science's correspondence to reality, as its ability to produce ideas that cohere together, that solve society's problems, and that advance democracy.

In all this, concepts of power are deemed highly important. For Rouse is at pains to balance an apparent liberality towards the project of defining science with a well-worked-out conception of practice. Although practices are to be understood as patterns that exist through continuation or repetition, they also involve norms, which are enforced – and therefore resisted. For Rouse therefore, practices "... always engage relations of power".

The force of Rouse's work therefore is to promote Hesse's pragmatic understanding of why we have scientific knowledge, with more continental, hermeneutical traditions, which exhaustively – and reflexively – establish the meaning of texts. In this context, of course, science is our text: we read its signs and construe their meaning. Those signs shift in meaning under the gaze of our understanding, and so we progress around 'the hermeneutic circle'. For our purposes, relations of artistic practice to scientific work provides ideal ground for the hermeneutic technique. The cultural analyst, or historian, or philosopher, properly sceptical of foundational epistemology, but nevertheless impressed by at least some of science's empirical pretensions, adopts instead a pragmatist view. He privileges nothing, but wants to see how ideas and acts, whether conceived as empirical or cultural, are actually at work in the production of scientific knowledge.

Two points are especially relevant for the interpretation of the following passages. Firstly it seems clear that a 'cultural studies of science' would be sufficiently plural to allow into its hermeneutical processes a consideration of art-science collaborations. In the excerpts that follow, therefore, the multitudinous views can be taken as possibly forming material for an account of science that emphasises beliefs as cultural and practical, rather than simply as increasingly true representations of nature.

Secondly, if we allow such considerations, then Rouse's interest in the norms, resistances and power relations of scientific practice would find immediate and urgent resonance in the current study. For apart from the examples I have given of scientist's engaging with artistic practice, there runs throughout these transcripts issues concerning the scientist's attitude to norms and power. The scientists are explicit in their self-portrayal of themselves as pursuing practices that in some measure set them in resistance to the prevailing sweep of the scientific project.

In what follows I trace out a possible commentary. This 'philosophical interlude' perhaps raises expectations that the scientists' comments that follow will themselves be largely philosophical and intellectual in tone. This is far from the case. These are comments that are rich in the detail of daily life, within and without the laboratory. Disparate and apparently personal to the scientists involved, they are samples from the transcripts that I consider sketch out a fine-textured picture not only of art-science collaborations, but of the scientific life more generally. It seems that interview questions about the place of artistic practice can provoke also a detailed account of contemporary scientific practice. Even if this is granted, in what sense can such an account be allied with Rouse's call for a hermeneutical or cultural analysis of scientific knowledge? A partial answer would be that these collaborations, and my questions, show up with some clarity systems of institutional and personal practice, and knowledge production, that are usually ignored by sociologists and philosophers of science. Discussions about teaching, about career advancement, about the problems of communication within the department, about time management and about scientists' attitude to public engagement, seem amply provoked by my questions. Such discussions, one might well argue, are likely to inform the kind of "edifying conversation" or cultural study of science, that Rorty and Rouse have seen as likely to prove the true foundation of a proper understanding of the workings of science. I shall leave to the Concluding Discussion further thoughts on how this work could be continued in future investigations.

III. The shape of the day

The 'epistemic space' of scientists is richly diverse, and is by no means restricted to the laboratory. Here Daniel Glaser explains how his day begins.

> 77 So a typical day would start at the coffee shop, I mean I think this is an integral part of my day, no I mean it quite seriously. I cycle to work, I leave the bike in the bike shed – so I've touched the lab - then I go to my favourite coffee shop round the corner, have coffee and depending upon the situation I'll either read the newspaper or I will read a scientific paper that I need to prepare for that days work. DG:7

The café is an integral part of the scientific day and is not best described as purely for leisure. In fact the cafe might be a place where the scientist is unusually free to pursue his craft. All my interviewees spoke of their day as complicated, their productivity imperilled by diverse demands. They are the demands associated with teaching, with money-gathering, and with research. The latter is described as what the scientist wants to do, and is meant to do. Teaching and grant-finding are distractions. Generally the interviewees were quick to point out that their irritation at such distractions had not yet persuaded them to close their doors. In fact these scientists describe themselves as rather undefended.

> 78 One of the problems is that if you plan to do some work here, ten to one there'll be people knocking on your door all day long, asking questions,

there'll be other members of the team, there'll be students, and other pieces of work come in, particularly by email. ND:52

79 Well I think at the moment it's [research] losing out because, if someone knocks on your door, it's much more immediate than a bit of research, or a grant, or something else that's going to mean something in the future, so essentially people always end up winning; so because we have quite an open door policy, teaching almost always wins out, at the moment, and I haven't quite learnt how to..... other colleagues do things like saying they are available two days a week, and the other days they just have locked doors, but I haven't quite...it doesn't really suit me to do that. MM1:117

80 Then I have a whole group of other research projects that have PhD students, research fellows, research assistants, those kind of people; so my typical day would be: fielding these people, these students. And, as you saw when you came in I have an open door policy, and they always come in. So I sort of drive their research much more than other supervisors might. So that's what I do. AB:8

These are scientists who see themselves as particularly generous. Just as I have been granted access, so too are students and colleagues – and artists. The scientists are complaining, but it is clear that they find it desirable to keep the walls of the scientific space porous. Perhaps (79.8, 80.5) these scientists see themselves as distinct in this way. They know the door kept open will slow the work: it will challenge the monk-like calling. There will be a cost, perhaps, in daily research productivity. But, preciously, it will invigorate the epistemic space. At least, this is what Daniel Glaser implies as he describes his work as 'scientist-in-residence' at the Institute of Contemporary Arts. The idea that artists might bring new ideas to the science is here related to space. Away from the lab, there may be a chance of coming across new and interesting people (81.4).

81 I think the place is what's important. I've been quite careful to organise most of the meetings at the ICA and not UCL or even the coffee shop around the corner. That makes it part of the community – for all the obvious things that happen in places: there are people that you meet, that come past. DG:632

When these scientists really want to think for themselves they have to seek another space entirely: an empty room in the building (83.1), or the kitchen at home (84.2).

82 And then I just work on my own research. In between. And sometimes I go and hide away. I don't do that very often, but I do have to do that sometimes. AB:13

83 Sometimes I hide up there because I know there's no-one around there; and sometimes I hide at home. AB:24

84 Sometimes I won't even come in, I'll stay at home and just sit at the kitchen table ploughing through these things. That's the typical day. RW1:22

These few comments evoke a working culture of clashing priorities. The focus of the comments was more the ordinary working day, less the peculiarities of the art-science collaboration. Yet in describing such daily tensions, these scientists presented themselves as unusually open to the time-consuming vicissitudes of social interaction in the scientific space. In the following discussion, Section IV, we see such openness emerging as an important factor in the early life of an art-science collaboration

IV. The start of the collaboration

Given the pressing nature of the scientists' regime, it is fair to wonder how an art-science collaboration can fit into the complex and 'adapted' work strategy discussed in the above section. Two speculations arise. Perhaps the collaboration is an idea dreamed up by the scientist when at last behind closed doors he or she begins to read and think. On the other hand perhaps it is the fact the door lies open to all comers that gives the immediate explanation. This latter possibility seems more likely. In the following, we sense the artist who is the most active. It is the artist who seeks out likely partners, perhaps encouraged by the idea of a grant.

> 85 The money that comes in from the Wellcome Trust means that friends, artist friends get in contact with me. RW1:221

Generally the artist is described however as quite persistent.

> 86 Well what happened was that she was talking to people trying to find out what was interesting to her. And it turned out that we chatted and I explained what we did and showed her some pictures of some of the aspects of the science and she got interested in it and so we decided to start a collaboration. TH:220

87 Andrew was slightly different. He already had his funding and remit; he came looking for potential collaborators, and I think I was just the person who was the most willing to sit down and talk to him. I can't remember how it started, but we hit it off on a kind of visual level, because a lot of the work I do has a very strong visual impact, always has done. RW1:207

We should not consider the scientists as passive, and simply alighted upon by artists. They are active from the beginning. There is a sense here that these scientists are used to moving around and working with people, and are intrigued rather than repelled by the artistic advance (excerpts 88.4; 89).

88 The ICA had asked me to be one of the judges; Alexa won, she was going round talking to the various judges, and she discovered me, asked me what I did, I explained to her, and she was immediately interested in visiting the laboratory, and I invited her. That's how it started. AL:8

89 And so Nicola[97] phoned me up and said do you just want to come. Just come. Come and see so you know what is actually possible, and can you do a little experiment. And you can have some space on this plane. So this was with not much time to go, not the equipment to do things, I had to think of something that would be relevant to all our ideas about joint loading, and be able to do it

[97] Nicola Truscott, Director of Arts Catalyst.

without choosing my subjects and with very small space without having much equipment. AB:278

90 It came about, I was sitting in a waiting room, waiting to go in to see someone, and there was a radio playing, lots of people sitting around, and some rhythmic music came on, and nearly all of them started to tap to the music. And I thought, that's interesting well we all know that that happens, but it's quite interesting, I wonder whether there's an innate pathway that's in place between the auditory cortex and the motor cortex that's influencing the excitability of the motor cortex. ND:630

91 And in dance, you know I went to the Royal Ballet, my wife took me to the Royal Ballet for a birthday present, and I was watching, it was around the time that this was going on, and I was watching the dancers, and they do not move at all, their bodies – I could line their head up with their background, and they were making far more intricate movements than just sticking their arm out... ND:205

There is a powerful narratival element to these quotes. Perhaps these intellectual links were not quite contemporaneous with the events themselves, but were developed later. What is clear is that these reflections show scientists developing robust and even dramatic accounts of how they are provoked by the arts (excerpts 90; 91) and how they start to work with artists (excerpts 88; 89). Whatever the tensions already present in the ordinary working day (section III, above), these scientists are open to accepting further challenges in the form of a liaison with an artist. It seems a remarkable feat. The possible rewards, intellectual and personal, must indeed loom up as significant . Perhaps too, however, it is relevant to attempt to articulate the prior attributes and qualities these scientists possessed, and that made the collaboration possible. This is the intent of what follows, in section V.

V. Are some scientists 'adapted' for this work?

I was interested to discover whether the scientists themselves perceived any quality in themselves that enabled the relationship to start. It was obvious to me during the interviews that these were people who might be able to deal with artists. I found them articulate in describing their work, generous in their access, and widely interested in the social contexts of science. Many excerpts hinted at the way these scientists see themselves as slightly distinct from colleagues. The excerpts below show the scientists explaining why they might be open to work with artists. Ian Thompson felt that the fame and the style of his supervisor was significant. As the inventor of bioglass, Professor Hench was successful, broadly experienced, and meshed in with diverse academic and clinical disciplines

92 My background with him [Professor Larry Hench], allowed me to spend some time sitting up from the trail of corn that we are following, and look around. I was partly down that road anyway, and then with Paddy[98] on top of that, I spend a lot more time looking around and talking to people that have absolutely nothing to do with my subject, talk to them about it, and they have ideas, and all these sparks suddenly fly and all of a sudden there is a whole new project there. IT:237

[98] Paddy Hartley, his artist-collaborator.

93 I was predisposed to art because I had lots of friends in the arts and I think that's to do with the Oxford college system... If you mix as an undergraduate with lots of people from history, art, English, they do the same job as we've just been talking about. I'm actually probably trying to recreate my college life, which I thought was the most intellectually fascinating part of my life. MM2:479

Miodownik looks back to Oxford, and in particular the shifting intellectual patterns of the College bar, as a kind of model. At the Oxford college, things were "intellectually fascinating": He would like the experience back. It is significant that, in his interviews, Miodownik constantly referred to education. In fact, the reason I interviewed Miodownik was for his leadership in setting up a novel curriculum development, called EngineeringArt, rather than for a science-art collaboration. This course involved undergraduate groups making successive visits out of the department, for example to the backstage of the National Theatre, or to Somerset House to talk to the designer of its fountain installation. In the following quote, Miodownik invites us to imagine that inspirational learning, even in science, takes place informally.

94 Because I'd be knocking about in the bar with people who could tell me about 17th century history and some thing; and I'd say that's interesting because I found out today that steel was, you know, and suddenly you are having this interdisciplinary conversation in the bar, but it's not forced, no-one is trying to get a grant out of it, it's just totally free and how wonderful. MM2:486

The loose and unpredictable manoeuvres of the art-science interface were attractive to Miodownik. They reminded him of what he had imagined academic life would be about, instead of the series of defensive fiefdoms he in fact finds himself surrounded by (excerpt 98). Similar points were made by the hepatologist Neil Theise. When I asked him to account for his ability to work with the artist Jane Prophet he listed a number of factors: his Buddhist faith, the fact that he has many friends in the arts, his Jewish origins, and his early interest in particle physics.

These scientists stressed the importance of professional contact with other people, and explained that this skill was not applied only when an artist arrived at the door. It applies to collaborating with scientists too. For example, Miodownik, a materials scientist, is interested in biomorphology. To start work in the area, he needed to discuss matters with biologists, and had to take some risks in finding them.

95 ...then I looked for people in London and said can I come and talk to you? Biologists don't do modelling at the cellular level, they do protein folding, so I said I do cellular modelling, are you interested? Most people of course never replied – they've got their own research – and "who is this guy?" – but Helen McNeil, from Cancer UK, just over the road from here said yeah come round to the lab... MM1:53

96 ...I think she thought I was mad, well not mad , but, just, basically, it's not that usual for people to pitch up on your doorstep. MM1:58

97 Yeah, well I don't know what she thought of me, she never really told me, because it was coming from me, the whole thing, like I was coming towards her. She was saying it sounds interesting; she was open to talking to me. We had a series of meetings over a couple of weeks, and I would show her progress I'd made. MM1:70

On many occasions in the interviews, the discussion shifted quite seamlessly from discussing collaborations with artists to collaborations with scientists.

There are complex institutional issues about what makes collaboration possible, or desirable. Within the art-science collaboration it is clear that the work is possible because there is some common interest, as well as there being very evident differences. According to Mark Miodownik, this interplay between sameness and difference explains why it is hard to forge collaborations within the immediate department.

98 ... in academic environments that's a problem, because there aren't that many people like anyone else; everyone seems to be different in their way round here; so I'm constantly – well, I feel like that – looking around for academics like me, probably I won't find them, because most people don't share my views. MM1:237

Miodownik suggests that, within the home department, each member of staff occupies – probably defends - a particular niche. It may be easier to find collaborators in other places. Those 'other places', it turns out, include for Miodownik not only other laboratories, but engineering companies, theatre design outfits, and even the Tate Modern. He is able to foster links most particularly through the institutional device of his popular course EngineeringArt (a course that replicates, perhaps, some of the processes he sees so beneficially present in the Oxford college bar).

Anthony Bull continues Miodownik's theme that an interest in communication might be a pre-requisite for the proper genesis of the art-science collaboration. No doubt rather like his College bar, Miodownik's descriptions in interview of his various collaboration, are very suggestive both of the element of chance operating at the beginning of a collaboration, and of Miodownik's ability to accept and work with the risk of failure. Here Anthony Bull comments on his own ability to cross disciplines and work with others. In the following excerpts note how 'speaking their language' (excerpt 99.2) looks more like a statement about the likely success of a collaboration than a comment on the mother tongue of international colleagues (excerpt 99.3). We should suspect too that the reference to international colleagues, 'engineers like myself' (excerpt 99.3) who are impossible to categorise, and apparently have a chameleon ability to elide into diverse professional spaces, is surely a description of Bull himself.

99 A lot of what I do is I go to orthopaedic conferences and I'm immersed in their world, I can speak their language, I understand them, I have a number of international colleagues, engineers like myself, who – some people don't know whether they're orthopaedic surgeons or engineers, they are that good at being involved in the orthopaedic world, understanding their problems, and contributing in that way. AB:68

We shall see in Section VI more examples of this overt positioning of the scientist as someone willing to embrace any number of communication problems as a worthwhile part of the collaborative process. Descriptions by Miodownik and Wingate of intellectual conversations within the department as liable to disappoint, remind us of the ambivalence towards the daily research life shown up by the growing art-science collaboration. The scientist who wishes to develop his own originality and his intellectual acumen, and thus his career, must look outside his department for assistance. Sometimes this will be a collaboration within scientific disciplines. The skills needed there, in terms of communication, will very likely be essential should the scientist expand his horizons so far as to encompass the practice of an artist.

The next section draws out some of the way these unusual scientists tried to describe what happened when they did indeed settle down to a project with an artist.

VI. The progress of a collaboration

Having started a collaboration, what was it like? In the interviews it was striking to see how often the scientists described their collaborations in terms of language: of talking to artists. In the excerpts below we see the scientists note the impact of their first conversations. Anthony Bull emphasised how odd it was that a 'facilitator' was needed (excerpt 100.2). The same scientist was specific about where they met (excerpt 101.1), and that the encounter was not only with an artist, but also with the 'outside ' (excerpt 100.1). The most frequent reference however was to the idea that to encounter an artist was in some way to encounter another language. Notice that in the excerpts that follow this aspect is developed quite fully. For example a central theme here is that the languages the collaborators spoke remained in place:

neither a temporary template (a pidgin) nor a third language (a creole) were developed. For the collaboration to work, the other's language had first to be mastered.

100 Well just because someone coming from the outside like that and saying I'm facilitating you two talking together is the weirdest thing I've ever heard of. AB:160

101 We went and sat in the meeting room down there and we chatted for a few hours – and we did not understand one another. We really didn't. AB:15

102 She also talks a completely different language, and it's very very hard … and also not only do I find the language hard to understand, but when I do understand it I find it hard to believe that what she is saying is correct. ND:289

103 Um, mostly we spent the whole time trying to .... I took it as language learning .... I had to learn the language, before I could understand the problem, and she learnt my language. MM1:79

104 Our languages were different, we had to learn each other's language, not a common language. NT:46

It is never easy to learn another language. Part of the difficulty is that the new language is not just a series of unfamiliar signs tied to familiar concepts. The foreign language describes to an extent a foreign culture, and the troubling aspects of this are seen in excerpt 102. Kitsou Dubois, a French dancer, gives Nick Davey problems of understanding even when a translator is available. When at last he feels he does understand what she is meaning, he finds himself assailed by a new problem: he feels she is wrong. In the interview Davey explained this further by describing a talk Dubois gave where she showed in video a sequence where a part of the dancer's body was entirely stationery. Davey was uneasy because it seemed clear to him that Dubois was tying a general principle to a clip from a video rather than a systematic set of data. The implication was that the video clip was selected to support Dubois' theory, and this went counter to Davey's view of scientific reasoning.

105 So she got for example two people holding hands like that, and the hands coupled together stayed still, and she put a little arrow on the video, and the bodies around them are moving around. I thought, when I saw it, and so did my colleagues, that's just chance. You know, she's been through all this video, she's found that bit there, and then she's made up some fascinating theory about it, and said, look. And I still think that. That's the truth of it. ND:299

Notice the reference to 'colleagues'. This is not some idiosyncratic obtuseness on the part of Davey; his colleagues shared his reservations. The artist Jane Prophet suggested that here is a problem that can be solved: "Both Neil[99] and I had to constantly check our meanings, not just words but concepts". For example, as we shall see later in this section, Prophet and Theise found that they had to confront each other on the traditional aesthetic term 'beauty'.

[99] Neil Theise, Prophet's science collaborator.

The problem of communication with artists seemed to elide more generally with these scientists views of collaboration. Excerpt 106 reminds us that the art-science collaboration is of course a diverse structure not restricted to the model of one artist and one scientist. A scientist might find himself working not only with one or more artists, but also with scientists from other labs and institutions (105.4). Davey's mention of the science colleagues who agreed with him, suggests an interesting, and complex, line of enquiry. When scientific colleagues collaborate together with an artist, what differences open up, not so much between scientist and artist, as between scientist and scientist (106.5)?

> 106 Part of these discussions centred on understanding one another. And so we had some sort of, we presented the way we thought about things, and the way we worked, the things we worked on to each other, in an informal way. And so we learned a lot about each other. And I learned a lot about the other scientists as well, because I didn't really know what they were doing; that was part of the story. AB:176

Alf Linney cuts across the tenor of excerpts 100-104 by suggesting that in these collaborations a common language can be found. Yet the style of his argument suggests he is symbolising the collaboration almost as a philosophy of life (101.4).

> 107 I often think that finding a common language, or common words, or a way of communicating, with somebody with an entirely different background, and therefore because of their background probably a different view of life...

enhances your own processes of thought, can only improve your way of looking at things. AL:242

All these scientists have described their honest efforts in facing and overcoming some problems in communication. On occasion they cannot resist demarcating their broad-minded attitude from that of more hidebound colleagues.

108 ...most scientists, when they talk to a choreographer, even movement neuroscientists, talking to a choreographer, would consider them to be talking absolute rubbish: they are talking about body memory, and all this kind of stuff, with no meaning whatsoever – just nonsense. I say no! I say these people clearly know a hell of a lot about motor learning and action observation, and motor planning, and all of this stuff you want to know about, it's just that they cannot say it in anything approaching a scientific way, and we shouldn't get them to try to. We should incorporate them into our daily work, so that their insights form part of our experimental design, and interpretation. DG:744

These are descriptions of struggles that are organic and shifting. In an art-science collaboration, there is talk but there is also practice. A huge amount of detailed information on ideas and skills are being brought together. They are spreading, wasteful and hard to organise. Yet the sense from these interviews is of the importance of substantial, if tacit, institutional support. In one space or another, arts concepts and science concepts are being put together, kept together and explored. The physical setting of these explorations vary, but it is clear that for the work considered here, these are efforts that are being bounded for a while within an institutional setting. There are meetings, conferences, grants and publications. These collaborations are extending far and wide, but they have a controlling element that removes excessive risk.

In the above discussion I prioritised the importance of communication, in particular the finding of meaning in each other's work. It is a strand that complements one explored earlier, namely the possibility of interpreting the collaborations of scientists and artists as based on the possibility of practising together in the science laboratory. But there is another element. Working with an artist might prise the scientist out of his laboratory: he might experience working in a novel location. This is an important element of the art-science collaboration, and the ability of the scientist to 'trade places' must be a sign of a successful collaboration.[100]

We saw in excerpt 26 the potent image of Hartley and Thompson standing together side by side at the glass furnace, casting their surgical implant. In the excerpts below, Anthony Bull (who like Nick Davey worked in France with dancers on zero-g parabolic flights) concocts a powerful image of a formal (109.1) project that is also weird (109.2) and complex socially (109.4)

109 So it was formal, structured, but then it was, I suppose, strange common experiences, doing weird things together, going on these flights, you know,

[100] The philosophical literature has on occasion seen merit in a notion of scientific practice as 'performative'. In a non-trivial sense, science is a series of actions, rather than for example a body of theory (Hacking 1983). If an artist and a scientist talking together suggests an interesting dimension for analysis, equally intriguing must be the prospect of artist and scientist 'acting' together, perhaps away from the laboratory.

stepping out doing weird things. And then its relationship, being able to work with some people and not being able to work with others. AB:406

In excerpts 110 and 111 Tony Holder describes the unusual situation of travelling to Africa, to the very clinics where he had taken blood samples and set up trials, and taking on this time the role of 'photographer's assistant'. In the interview he described with feeling the novelty of being in a familiar space where ordinarily he was the busy and important scientist, now 'reduced' to minder and tripod-holder. His role in that familiar space was utterly transformed, thanks to the artist. It was a temporary transformation he found interesting and useful.

110 Even though I had visited before, it meant I could go and get out into the houses, and see the mosquitoes first hand, and go to the breeding grounds, and all these sorts of things, and actually interact much more with the communities that are being affected, than I had previously ever had the opportunity to do so. TH:257

111 So I found this immensely valuable, for me, to actually have what we do put in that perspective, outside the laboratory, if you like. What some people might call the real world. I think they are both real, but you know what I mean, the sharp end as it were. So that was valuable to me, and I think that is the enduring sort of important experience that I've had from that whole thing is that I've acquired that in a much stronger sense of that, if you like. TH:261

We can see now that the question of how a scientist might 'benefit' from working with an artist is itself capable of endless transformation. The quotes being interpreted in these sections show scientists reflecting assiduously on the implications of the relationship with the artist. These reflections supplant now the simple considerations of direct assistance that I adumbrated in chapter 5, section III. Now we see scientists reflecting on the 'weird' and surprising happenings of the collaboration, and positioning these reflections in relation to their professional identity. In one intriguing comment by Tony Holder, the scientist commented that his collaboration with the photographer Zharini Bhimji had left him looking at his laboratory glassware 'differently'.

The elements discussed in this part begin to provide, I suggest, important resources for the argument that the scientists are 'changed' by these collaboration. The experience of working with an artist has not been so peculiar as to take the scientist entirely out of his normal mode of operation. The powerful aspect of these collaborations is more that they shift the scientist to the 'edge' of their normal scientific life, but not beyond. To an extent, they stay within a recognisable, scientific zone. Tony Holder may be carrying the cameras and the tripod, but he is carrying them about the clinic that some months earlier was the site of his research. Thompson may be rehearsing the lost wax method with a master sculptor, but it is in the context of an attempt to make better casts. And though the experiences on Zero G aeroplanes were seen by Anthony Bull as 'weird', those experiences were nevertheless scientific experiments. It is this tendency for the collaboration to stay within the professional orbit of the scientist – just – that the interviews tended to exploit as a key focus for discussion.

It is interesting, in concluding this section, to consider a comment by Mark Lythgoe. Without referring to science, Lythgoe ascribes to his collaborative work with Kötting the cause of a fundamental shift in his own persona.

112 Working with Andrew, seeing him always just be himself and saying well, people have got to accept me for what I am, and being slightly non-conformist, gave me an awful lot of confidence to be Mark Lythgoe, and I would say I found Mark Lythgoe through Mapping Perception. ML2:29

113 But how it's changed me most of all, I'd say its given me the confidence to be myself. ML2:43

VII. Costs, benefits, calculations

I found as interviewer that so long as the questions gave scientists the opportunity to refer back to their familiar scientific life, apparently limitless lines of enquiry were possible. For example, it seemed to me interesting to find out how the scientists viewed the career implications of the work. As we have noted, in so far as the science studies literature engages with the motivations of scientists, it tends to favour analysis of credit-gaining in the profession. There was no doubt the scientists would follow the sociologists in themselves asserting the importance of publication and grant-getting, and might well confirm that the world of art-science collaboration fits uneasily with those structures. However – and this was perhaps my main reason for pursuing the theme – it seemed to me that these scientists had asserted their distinctiveness so often in the interview that perhaps they would be more than happy to lay out sceptical views about the standard model of scientific credit.

The first point is that a 'sciart' award is to the scientific mind quite small. Anthony Bull described his experience of failing to win an application for one such award.

114 Big frustration, really. Anyway. And again, its peanuts! I mean all the other grant applications I do are for huge sums of money! AB:356

115 I can't remember, it was about £20,000. So it really was peanuts. It didn't pay for anyone to work on it, pocket money. But it was great. AB:173

These collaborations have the effect of throwing an unfavourable light on some aspects of the daily job. Mark Miodownik here expresses his alarm at finding his intellectual stimulation circumnavigates his department. Within the building, things are dull.

116 ...it's made me realize that I have more interesting and vibrant intellectual discussions outside the department than inside the department and that's not right, I don't think that's right.... MM2:299

Working with artists might cast your colleagues as apparently uninteresting, and show up your ambivalence about a threatening (117.8) and impoverished (117.9) environment.

117 If you expose yourself to people outside your specialism which is what scientists in a way hate to do, you get asked awkward questions and you have to confront them; and you get asked why are you doing this? Why to the nth degree are you trying to work out why this bit of material does this, when in fact I'm interested in making a whole new material. And I have to say to them, well the thing is that won't produce a publication. And then you realize, by responding to that question, it exposes your modus operandi of how to survive

in a science department and you realize that to me it doesn't seem like a very rich environment. MM2:320

The people who are challenging Miodownik are his collaborators: artists, architects, theatre designers. These people have helped him see better that the rules which he works by are actually constricting.

Anthony Bull saw as an issue the way artists lived in a professional culture less secure than his own. If we have seen artistic practice as something these scientists are prepared to interact with, and prepared to benefit from, whatever the problems with communication, the following excerpt from Bull alerts us to another set of issues.

118 [The collaborating artists] are not academics, they are not artists at universities with lecturing jobs with a lot of security in their lives. So that insecurity in their lives is reflected in the way that you do work together, it really impinges on it. They don't have, they are rushing here there and everywhere. They are suddenly are out of action for six months because they have got a project and are doing something; or they have to do another job for a while to earn some money to allow them to do what they want to do. AB:501

Bull raised this difference as having a bearing on professional ethics.

119 And that's something you have to be careful about. Because I'm in a very different situation to them. My investment in it will affect me professionally as

> well, but it affects them far more. It's their living. For me I still have this living. So I think that is responsibility on me that should weigh quite heavy. I mean it's a responsibility and you should be serious about it. Yeah. AB:508

Bull has noticed that these art-science collaborations raise issues of power. It would not be too blunt an assertion to suggest that a frequent feature of the art-science collaboration is a gender constellation where a younger (freelance) female artist collaborates with an older ('tenured') science academic. Beyond noting this pattern, I have not felt able, in the current study, to explore systematically how gender and professional security may be distributed across the dozens of project funded (for example) by the Wellcome Trust.

We can see in Bull's comments however a further intimation that this section's interest in 'the scientific self' might indeed be a valuable organising principle. Not only does Bull see himself as secure and well-funded (so that £20,000 is 'peanuts). He has considered the implications of working with someone much less favoured in terms of institutional largesse. It is not necessary to draw an explicit analogy with the conventional marriage where the male is the bread winner, and the female is the home maker. Nor shall I pursue the notion that gender politics casts an especially sceptical eye on the assertion that the female home maker is by no means a loser in the relationship. Yet Bull's point alerts us to a possible interpretation of the artist as 'the weaker partner'; and we have seen too in other excerpts the idea that vulnerability – something perhaps more readily accepted in artists than in scientists – might well be something the scientists see as a valuable aspect of their artist colleagues (excerpt 49).

It is intriguing then that these scientists often spoke of their art-science collaborations as opening them up to criticism – as making themselves vulnerable. The problem is not simply a slight philosophical mismatch with the 'Departmental view'. They spoke of serious, career-threatening, implications.

120 I think if I'd not done the science and art, and not done the science communication, I would have furthered my science career, and found it easier to get the grants in. There's no doubt about it. I've put less time into the science than other people, and I've paid a price for that. And I get really depressed about that some days. I've definitely made a terrible error of judgement from that point of view. ML2:18

121 It obviously doesn't fit in terribly well with your career because clearly what is expected of scientists, at the moment at least, is not to do sci-art. So in some senses unless you can show in a very concrete way its contribution to your field then it may be regarded as a negative thing by your peers. So this is a problem immediately. AL:165

How might the scientists cope with this problem? One strategy would be to try and persuade your colleagues of the value of such work.

122 When I talk about Engineering art, in different departments, I get this puzzled look on everyone's face; they go: "yeah I know that in the past artists have helped materials scientists design new materials but they are never going to do

it in the future, and since that's what materials science is about, what's the point?" MM2:117

123 Then I have to say quite vague things like creativity, creative thought, vibrancy of the department, you can't really know, there aren't a finite number of materials we are trying to discover and we are just having to plod through them; materials are for a particular desire, and if you don't know about desires, then how are you going to design a material, and artists are very good at designing... its all vague. MM2:122

124 If I was in Imperial I'd probably be told to stop doing this. But at Kings I havn't been told to stop doing it. It's very much – "you can do this Mark, it's your hobby, off you go but don't let it interfere with your real work". This not being real work. And that frustrates me. What it means is that I really have to make a case for it. And bide my time. Because I think its time will come. MM2:285

125 But if someone is looking over my shoulder, saying what are you doing, time and motion, all this, you shouldn't be spending time on it, but anyway nobody does so I don't care. AB :357

Again, we see the scientific self being defined through exploration of the implications of an art-science collaboration. Mark Lythoge (120) and Alf Linney (121) are blunt about the dangers the art-science collaboration can pose. Mark Miodownik (122-124) and Anthony

Bull however are prepared to stick it out. Miodownik will "bide his time" (124.5); Bull simply ignores the problem (125.3)

If money and colleagues are a problem, another is the issue of scientific publication. The science paper is extremely limited in its repertoire of styles. Richard Wingate enjoys the science journal style, but recognises it might foreclose on certain creative tendencies in the art-science collaboration.

126 That really does encourage this kind of, the producing work for objective assessment, independent of personality. Maybe you should be personality-less, I don't know. RW1:687

127 The monkish aspect of being a scientist, though, is I think quite … there is at the core of it, the essential part of it, I suppose in science you live or die by your public published work. In that respect, whether you do sciart stuff till you are blue in the face it doesn't matter, it is your publications that count. RW1:683

It is an easy matter to grade a scientist, and his department, according to publication rates in high-impact journals. The value of a science-art collaboration eludes such scrutiny.

128 Well the difficulties are measurement, measuring output, measuring success, measuring failure. And particularly at Imperial, for the past few years, and coming up, we are being measured for our success and failure. And it's still not clear what those parameters are going to be. It's going to be publications,

it's going to be grant income, it's going to be loads of different things. So I don't know how these things can be measured. AB:492

These scientists are aware that the art-science collaboration may take them outside the standard trails of scientific credibility. The small amount of money involved will hardly figure in the departmental metrics; and the typical science paper's championing of such procedures as the control test, the double blind, the random sample, and statistical analysis, may severely limit the scientist's ability to direct the fruits of his art-science collaboration back into the department's cycle of credit.

Once again, we find the scientist's ambivalence about the effect of contemporary publishing culture explored via the art-science collaboration. The quotes above show scientists in effect ignoring the problem. They rise above the petty and simplistic metrics of contemporary science, and pursue a collaboration where the fruits are not so simply measured – and all the better for that. One of those fruits, these excerpts suggest, is a development of the scientific self such that new ideas and insights become more likely. This is how Anthony Bull makes the point:

129 I don't know. It's going to allow me to more effectively do research because I have a wider experience. It's very important I think to have wider experiences, rather than always just answering narrow questions that are incremental. And maybe from that come bigger leaps of understanding, or bigger questions anyway. They havn't yet, but you are only going to get that if your experience is wider, if you have some broader interaction, which is why people like to go to conferences. Well this is like going to conferences. It's like going to

> conferences with people who don't speak the same language as you, and who deal with different things. But, they are dealing with the human body, I'm dealing with the human body, they have an experience of the human body, I'm talking about what the human body is doing. The overlap is huge, and we never ever come together. AB:455

I made clear in the literature review that a major justification for the Wellcome Trust's funding of art-science collaborations is the way these projects can feed into current developments in the 'public engagement with science'. While I have been at pains throughout this thesis to underscore the possibility that an art-science collaboration need not, and frequently does not, carry an explicit 'public engagement' product, many of the projects I mention do indeed link up with public shows and events. Indeed, in the excerpts that follow, we find the scientific self being elaborated precisely by the opportunities offered by the public stage. As we shall see, scientists varied in their wish to use their art-science collaboration as a vehicle for going public.

VIII. The issue of public engagement

All the scientists noticed the implications of their work for laboratory publicity. Typically these reflections retained a complex and ambiguous view about the manner in which the art-science interface was fertile territory for some kind of contest between the institution's need for publicity and 'public engagement' kudos, and its tight supervision of income and career metrics. The scientists were prepared not only to describe a potential conflict or even a hypocrisy. They attempted to situate themselves within this matrix of forces.

130 That's one of the ways I justify this work. The EPSRC for example had a splash on our things at the Science Museum. Although they will say we don't count this sort of thing towards the RAE, they will nonetheless exploit it for their own purposes, when it comes to the publicity they want it as well. So actually getting that publicity is quite important. And those leaflets from the EPSRC were around the building, and around the college, the college made a thing of it. They do actually benefit from this, and recognise the fact, even if they won't say so in words. AL:187

Dealing with the media, or becoming a regular on Radio 4 and science festival round-ups, has implications for a scientist's identity. Mark Miodownik had reservations about this kind of work.

131 I really have to fight my ego. This whole area brings out my ego. You really start thinking...if you are not careful. You have to be clear – you are helping the community of scientists. You've got to make that very clear. MM2:26

There are a number of related subtexts here. Most specifically, the Wellcome art-science scheme was always part of the organisation's mission to explain its work to the public (Turney 2006). A broader point is that this ambition, shared by many science institutions and by government, is hard to realise and hard to evaluate. In terms of recent history, the conventional viewpoint is that the allegedly patrician tone of the original Royal Society enquiry into the public understanding of science (Bodmer 1986) has been replaced by a more dynamic and democratic 'dialogue', where scientists and scientific institutions engage with and listen to the public (House of Lords 2000).

Within the relevant academic circles, the completeness or even the intention of the substitution is often doubted (Irwin 2006). What cannot be doubted is that cultural currents now run through science institutions, inviting scientists to get involved in acts of public engagement. A recent manifestation of this is the setting up of 'Beacons of Public Engagement' within higher education institutions, co-funded by the Wellcome Trust and by the Higher Education Funding Council for England (HEFCE).[101]

The interviews revealed complex judgements made by these scientists about the authenticity of these developments. While clear about the value of their collaborations, the scientists varied in how they approached the questions of public engagement. Richard Wingate will collaborate with artists, but he isn't interested in publicity (excerpts 132.1; 133.5)

132 I don't want to be in the Observer, and people to come in and read about it on Monday morning here in the lab... to be tagged as a science-artist in the science world is very bad. RW1:744

133 I didn't tell anyone about Head On. They just found out about it as it went along. I'm very happy to apply for as many sci-art grants as possible, and Id love to do more with Andrew, but I'm not going to ... its not going to be... Andrew can come in here and he can spend as much time talking to me as I want, but it's probably not going to be something I want to post up. RW1:664

[101] Wellcome website http://www.wellcome.ac.uk/doc%5Fwtx037034.html accessed September 17, 2007.

On the other hand, it would be a mistake to see these science-art projects as simply attracting the attention of an abstract and distant public that science institutes are uneasy about. The 'people' who Wingate is catching now, as they wander down his corridor or past his website, are neither faculty nor laity: they are proto-scientists and apparently promising ones (excerpts 134.2; 135.6).

134 Probably on the web-site, if I posted it up there, which I probably will. It might attract a broader-thinking person, so it's got some utility. RW1:682

135 Although there is a small value in it, in that it does get people, the stray people wandering past, interested, wandering past on the internet, wandering past down the corridor looking at your poster of your work. It does catch people's interest. Just at that basic level of noticing the work that you do, be it potential postgraduate students, that's usually the best one actually, you can attract more interesting ... RW1:668

These interviews show up some distortions in the concept of public engagement. Mark Miodownik exclaims here that designers have a clear understanding that *their* projects have people at the centre (excerpt 136.1). In other words, for a designer, the value of your work will depend on your knowledge of human needs and human acts. For Miodownik, the same imperative applies to materials scientists. This is why his course EngineeringArt takes undergraduates off-campus to meet designers and architects. It is not simply that he wants to reach the public, or enthuse them with the facts of science. It is more that a properly functional engineering academy will find ways of discovering what it is that people want of machines and of materials. He compares design products and designers with the conventional world of materials science (136.4).

> 136 And they are about people! Science, especially the kind of science I do, materials science, is essentially advertised anyway as ... it's got nothing to do with society people or culture or anything like that.... I mean if you find a materials science textbook that mentions a person and their needs, good luck to you. MM2:279

The sphere of public engagement, according to these excerpts, provides both opportunity and danger. The danger is that such work takes scientists further away from the science publishing machine, and casts the art-science collaboration as some kind of extra-curricular activity. The ability to judge that danger is acutely necessary for these scientists, given that university press and communication offices are liable to view such projects with interest, and thus encourage scientists to seek credibility for their art project through media exposure. The opportunity is not so much to do with the abstract notion of dealing with the public, as with the benefits to be gained from turning the scientific self into something outward-looking and socially-engaged. That kind of engagement need not mean with a Radio 4 audience. It could be manifest in the way a scientist runs his lab and recruits his staff. For example working with artists, according to Wingate, can reach deep into your laboratory organisation. He attributes to his work with Carnie a baffling and enjoyable tolerance of a colleague's peccadilloes:

> 137 I'll tell you one thing; it's enabled me to recruit better people; N. is off now to do homeopathy in a psychiatry outreach place in Highgate. She said I do this

> on Wednesdays when she joined, and I said that's fair enough; and its baffled me slightly that it's never actually bothered me, in the way that it would bother a lot of my colleagues. They'd be furious if someone came up to them and said not only am I not going to be here on Wednesday afternoon, but I'm going off to do something like homeopathy... RW2:428

Excerpts 136 and 137 provide a summing up of this part of the thesis. They describe scientists quite fervently wishing to make the laboratory less isolated, less 'Latourian'. This is not the kind of public engagement we generally hear about. Instead it is a form of communication where the laboratory takes into itself something of the plurality and idiosyncrasy of the outside world. Miodownik (136) wishes to find expression in his laboratory for designers' struggles to find something that works for a paying public; while for Wingate (137) it is worthwhile to find in the laboratory routine space for someone as heterodox as a homeopath working in "... a psychiatry outreach place in Highgate".

These are significant developments in the scientists professional identity. Once again, the scientists are attempting to describe the consequences of their work with artists in terms of their scientific life. In the discussion chapter all these aspects will be reflected upon, and some conclusions drawn. Finally however, let us turn to some of the words of the artists themselves.

IX. The artist's voice

This thesis has the words of scientists at its centre. We learn about the artists from their scientist collaborators, not from themselves. It would be a second study, and an important one, to interview the collaborating artists in depth, and obtain their perspective. Yet during the course of this study, I spoke often to artists, and formally interviewed four of them. What follows now, the last section of the chapter, is a brief account of some of their views. A more extended study would no doubt uncover interesting aspects of artists' account of their own discipline, and of science. In the meantime, however, what is of note in the excerpts that follow is confirmation of the key role the artists have played in structuring their collaborations with scientists.

The first excerpt however comes from a scientist, Mark Miodownik, who sees artists as more sure-footed than scientists at moving in unfamiliar territory.

> 138 The one thing I am always very impressed with is their intellect, and if they've done anything with scientists before they know how it all works and they're much more knowledgeable than the scientists are outside their discipline. MM2:441

Perhaps echoing Miodownik's appreciation of the artists' acumen, the most noticeable aspect of the interviews was the way the artists made significant challenges to the scientist's work. Jane Prophet for example discussed at length her science collaborator Neil Theise's attitude to scientific images.

> 139 I say to Neil – he's got forty tissue slides, they're all quite similar – why did you choose that one for *Nature* magazine, or for *Blood*, or whatever. And when I first asked him the question, he didn't understand the question. He just said, what do you mean? I said well why did you choose that one, and not one of these? And it turns out that on probing he preferred the composition, but to

be honest he didn't even know that. It's not even conscious. When I ask him to tell me what processes he went through in selecting one image over another, when I first ask him that question he doesn't understand the question because it is not a question he has ever been asked, or asked himself. He's never been trained to ask that question. He can answer it in terms of, oh the fluorescent dye is clearer. But actually, the one's he's kept in the packet, the fluorescence is fine on all of them. JP26

140 I'm very questioning of it. I was trained in the 80s when, as now up to a point, there was a lot of resistance of beautiful images for their own sake, a lot of suspicion of that way of working....There was a lot of engagement with conceptual art, and art as a tool for thinking, and therefore however much I might want to move away from that when I look at an image it is never simply a pretty picture, it's always a tool for thinking... what I've become aware of in science, which is completely different from art practice, is this idea that an image can be objective. Which we would say in the arts, particularly from phototheory from the 1970s, no image is objective, no war photograph, nothing is objective – because it's framed, it's composed, and that's before its cropped, its contrast is increased.... so the idea that all of that is not brought into play I find really problematic JP:39

Prophet and Theise were not discussing a work in London's National Gallery, or even a work from Prophet's portfolio. The artist and the scientist were scrutinising professional decisions made by the scientist. But the most potent impact on Theise was not Prophet's art theory, but her artistic practice that draws on ideas of artificial life and complexity.

141 I said to Neil, I want to make a film about complexity theory. So I described the ant walk theory... Neil was captivated. It turns out that ants were fascinating for Neil in his early childhood. The trail was a product of emergent behaviour, not of an individual ant. Neil had watched all this, for real, as a child. JP:82

Jane Prophet had read some of Neil Theise's papers on stem cells and "sensed a connection". The connection she sensed was that the dynamism of stem cells had some link with emergence. She suggested to Neil Theise that 'stem cell-ness' was an emergent quality, not a thing.

142 Neil [was] partly horrified, but begins to think it through – this is a mad idea, could there be any truth in it? JP:91

The artist Marilène Oliver was also exercised by the meaning of scientific images. Like Prophet, Oliver is inclined to challenge the scientists on their own ground. She has noticed the proliferation of scanning technology within medicine. She asks whether the ubiquity of 'the scan' has political or moral consequences. In particular, what do they do to our sense of our bodies?

In an interesting echo of Wingate's reflections on his own methodology[102], Oliver argues that because a scan is a slice, and is so frequent an accompaniment to a visit to a hospital, a fragmented view of the body is being naturalised by modern medicine.

143 MO You can't see them all at once. You can't situate them. They are out of context. You can't understand them really.

SW Why does that matter? ......

MO It's a loss of ownership of your body. And the knowledge of your body. And the perception of your body. It's so far removed from you, it doesn't correlate at all with what I know of you. MO:69

Oliver sharpened the point still further by challenging the working culture of the clinicians who interpret the scans. To the argument that scans help us diagnose disease by virtue of their narrow focus, Oliver replies like this:

144 ...the lived-in body is lost. Some doctors would never actually meet the patient face-to-face, but only look at the scans. You lose that human contact, the humaneness. MO:78

Marilène Oliver's response has been to 're-build' the body by making scores of scans head-to-toe of a single person, screen printing each image onto transparent acrylic squares, and then mounting them into a tower. What at first resembles a plastic block contains in fact a

[102] See excerpt 46, p185.

complete person, represented by his scans. The scans, as it were, are built back into a human being, rather than being hooked up on a white screen. They make us think about humans, not about disease (Webster 2005, p965).

Another artist I interviewed, Phoebe von Held, was less concerned with scientists' assumptions about images, as with their representations of the future. Her interest was in directly engaging with their perceptions about the future of their field. Von Held centred her debates with her scientists on the work of Diderot: the Enlightenment encylopaedist impressed by the possibilities of science, and inclined to speculate on what the future might hold.

> 145 I was hoping I could get scientists to talk about their more visionary dreamlike ideas about where research might go in the future... but I didn't get so much. PH:29

In particular, Von Held followed Diderot's lead in wondering where science's study of consciousness will bring us.

> 146 In D'Alembert's Dream there are questions about dreaming, consciousness; so that was another question. I asked everyone what is consciousness. Especially the neuroscientists would answer. They'd say that is a conversation about the pub, or something for very good scientists that at some point give up their career, it felt like a taboo. PH:53

Faced with the example of Diderot, the scientists fell silent. They could not give an opinion, at least not while they were sitting in a laboratory (excerpt 146.3). Von Held explained this reticence as following from their desire to be reliable experts.

147 I guess they are professional researchers, they want to be experts. I can understand that, I wouldn't want to talk, I wouldn't want to be interviewed, they are professional, they are not trying to be experts in what they are not. There are only very few people, people like Diderot, who wanted to get a grip on what holds everything... PH:96

Yet the scientists who worked with von Held were in other ways very active in the collaboration. She firstly asked the scientists to read her Diderot text, and was interested in how they responded.

148 With Robin I wanted him to talk about cloning, they didn't have to make that so much of a leap, to go back. There were some who became very interested in the text itself, David Wilkinson for example, he went back and read the entire text, and read up on 18th century medical history on preformationism, we had these two hour meetings where we literally went through, and then Dimitrious Kioussis who read the entire text, who didn't want to be interviewed or recorded or videoed but who read and annotated the whole text...they were very interested in the text. PH:12

If the scientists Von Held interviewed were elusive on future directions, they were extremely lucid on their work in hand. Von Held described them as impressive communicators.

149 [They were] extremely clear and eloquent communicators, very clear communicators, just very clear minds. I was very interested; I am very interested in differences of diction; I wanted to dramatise them; they all had very different way of expressing themselves but all of them speak in an extremely clear language... they could make it clear for me. PH:63

The style of that clarity became material for the artist.

150 ... I always like it when you can hear people think while they are speaking, and Diderot has that in his text, and obviously the scientists have that when they speak, they talk about very complex ideas, yet you can feel that they are constantly re-configuring in their minds what they are going to say, and I find that dramatic quality of speech in itself. PH:203

Phoebe von Held mentions here her appreciation of the clarity of the scientists in describing their work, even if they became tongue-tied when the discussion moved too far from a familiar area. Yet a striking aspect of my interviews with the artists was the clarity with which they were approaching the scientists. The artists are very clear about the work they want to do. They are not simply in the orbit of the scientist, waiting for ideas to spin their way. If Von Held wanted scientists to discuss the future, Marilène Oliver needed an MRI team willing to implement her ideas.

151 [When making Family Portrait] we all went up to Nottingham, [I had] blanketed every MRI Department I could find in England, please would you

consider, and Nottingham said yes, so we all went up to Nottingham. Because I had already made the sculpture "I know you inside out" I already knew the system, I knew what I needed was the actual scans every two centimetres, that's what I asked for, of course they don't do this, there's no reason for them to do this.... Normally it's a much finer slicing: they'll just be looking at one part, they might just be looking at my back, they wouldn't do a whole body scan... MO:89

This doesn't sound as though the artist is being re-configured as a scientist. In fact, looking at Oliver's sculptures, an uninformed eye might indeed construe them as produced by an MRI machine, with the artist on hand as an illustrator finessing the work for the purposes of exhibition. The artist-as-illustrator was not however a model welcomed by my interviewees.

152 [I'm] uncomfortable about working with science so as to be an illustrator, uncomfortable with Wellcome Wing art... art should be purposeless. Often it works very badly, this science art, [I've] lots of problems with that kind of work. JP:69

A statement like this is very suggestive of further work into the motives, and goals, of artists who work with scientists.

Chapter 7: Concluding discussion

At the end of the literature review I laid out my research questions. Put most simply, these questions asked: 'for a scientist, what is the point of collaborating with an artist'. The context for the study as a whole was the burgeoning in the last 15 years of what seems an unusual experiment within the scientific profession, namely the extensive funding of projects involving both artists and scientists.

The literature review attempted to fill out the background to this study. This review showed how long-standing arguments about epistemology can help frame an enquiry into the role art might play in the scientific enterprise. In particular I drew on the tradition of social constructivism. I showed how positions we associate with the strong programme,[103] and with laboratory studies, might cast a sympathetic light on explorations into how artistic and scientific practice can work together.

However it is a series of interviews that forms the heart of the study. Analysis of those results, I suggest, settles into three sets of findings. Firstly there are examples of artists (or artistic practice) being of direct utility in the work of science. Secondly there are cases where scientists deepen their metaphysical understanding of science (reductionism for example) through their contact with art. Finally there is a large set of cases where scientists show themselves able to discuss their professional culture as though it has been impacted upon by an experience with art. In what follows I shall attempt to assess whether these sets of evidence are indeed signs of science, or scientists, being changed by art.

[103] See footnote 4.

As regards direct impact of art on science, it was the example of Ian Thompson and Paddy Hartley that I highlighted. There, the sculptor took a direct part in the preparation of Thompson's glass casts. The example is drawn from the world of medical prosthesis, and from a hospital tower in South London: it is a case study not instantly generaliseable. Yet, I argued, here was an instance of a scientist enabled to develop his professional skills through the attentions of an artist. The artist had led the scientist to do things differently.

I derived broadly similar arguments from the experience of Richard Wingate and Philip Kilner. For Wingate, whose daily work involves the scrutiny of images of cells, there was utility in artist Andrew Carnie's visual sensibility to cellular orientation and topology. Wingate and Carnie discussed the migration of cells, and according to the scientist the conversation was fruitful. Philip Kilner, the cardiologist with an arts background, could put the two aspects of his training together as he analysed the scans of patients' hearts. His intimate understanding of form, gained through the training as an artist, acted as an important facilitator of his clinical understanding.

My analysis of the working patterns of Thompson, Wingate and Kilner addresses therefore the first of my research questions (see p107) and provides an indication of direct impact on the daily research practice of a scientist. Yet it must be admitted that these examples are in some measure extraordinary. The cardiologist who is also a trained artist is surely a rarity; and there can be few developmental neurologists who sit down with an artist to examine micrographs of neurones.

Nevertheless the force of the transcripts is that, however unusual the phenomenon may be, the actual process by which the artist and the scientist worked together has a familiar ring.

That is to say, we can recognise from the interviews that Wingate and Carnie were doing work easily aligned with the task of the laboratory: they simply sat down and looked at neurones, and discussed what they saw. There is something *ordinary* about this. Similarly, when Thomson and Hartley worked together on casting glass to the shape necessary for a colleague surgeon's operation, they were engaged on an exercise in essence routine for an orthodontal section of a London hospital. The transcripts suggest, therefore, a paradox. Though in many ways extraordinary, it is the way a collaboration can elide with the ordinary life of the laboratory that makes it potent, and a vehicle for change.

My second research question complements the first by leading the study into broader reaches, in particular provoking analysis of what the transcripts reveal about scientists' intellectual and cultural concerns. For example, on the role of reductionism in contemporary science, the scientists were liable to relate their experience of artistic practice to their own attempts to force a broader perspective into his or her work. Thus, Davey, Kilner, Wingate and Linney all voiced their concern about 'over-specialisation', and implied that an arts collaboration might have as one of its effects a welcome corrective to the reductionist project. The example of Nick Davey is a reminder of why it is that artistic practice might stabilise a scientist's ability to balance his reductionism with a more holistic vision. Through working with the dancer Kitsou Dubois, Davey was examining posture not simply as a matter of neuronal control, but as something principally expressed by the whole human being.

The claim that an experience of artistic practice has prompted a scientist to engage in some reflections on metaphysics receives further support from Wingate's discussion of his views of scientific evidence. We saw that Wingate related his appreciation of the 'frailty' of scientific evidence in part to his work with Andrew Carnie. Like Wingate, Carnie was interested in the freeze-frame photography of Muybridge, and in the 'partial' staining techniques of Ramon y Cajal. Through time spent working together, Wingate saw more clearly how his own sampling of neural material raised issues of evidence more obviously flagged up by the procedures of Cajal, and by the photography of Muybridge. That is to say, Carnie provoked or triggered a more thorough understanding on the part of Wingate that his 'narratives' of the travelling neurone incorporate the same kind of gaps that allow freeze-frame photography to be transformed into the seamlessly moving image.

These two sets of examples, relating to practice and to tacit 'philosophy' are compelling, complementary indications that the artistic element can be a powerful factor in the work of a scientist, and be 'an organiser' of change. And I suggest that, so far as the collaboration does have this significance, it depends in part on the process of working being in some measure recogniseable, rather than alien.

Research Question 2 is explored yet further in the second of my analysis chapters where I traced out diverse elements from a very broadly conceived interpretation of scientific culture. In this chapter are comments on matters as diverse as café life, colleagues, the nature of communication, and the public engagement with science. I have shown that such cultural issues, under discussion in interview, frequently elided with reflections on a scientist's collaboration with an artist. I suggest that the transcripts invite us to see those broad cultural issues as entirely germane to the scientist's professional life, and hardly separable from some 'epistemic core' imagined to exist as the abstract essence of the research laboratory. Of course my discussions of recent sociology of science facilitate the reflection that (for example) Daniel Glaser's fondness for café's, his work as scientist in residence at the Institute for Contemporary Art, and his diary schedule as an image analyst at University College London dissolve into each other as factors in the co-production of scientific knowledge. It is a crucial part of this thesis that the same point is made – perhaps more forcefully – by the content of the transcripts. In other words, the scientists saw in great detail how their dealings with artist colleagues impacted upon, and changed significant aspects of the scientist's professional culture. Thus Richard Wingate credited his experience with artists as a cause of his liberal approach to a colleague intent on practicing homeopathy; and Mark Lythgoe, in describing his own collaboration as helping him 'find the real Mark Lythgoe', pointed to a shift in the way he communicates with colleagues as a permanent change. Many other examples were discussed in the analysis chapter.

All this leads us to accept the argument that art can step into the orbit of science, and organise an effect. In terms of practice (both practitioners working together), metaphysics (views on reductionism), and culture (the wider aspects of a scientist's professional world) my study finds the artist collaborator entering into the scientific enterprise. The power of the argument relies in part on it being three-pronged. The actual examples of co-practice are complemented by art's impact on a scientist's understanding of metaphysics, and his understanding of his professional culture.

Nevertheless, turning now to research question 3, the transcripts confirm that scientists are liable to embrace the empiricist model I described at the start of the literature review. Several of my interviewees, faced with a query concerning the possible 'influence' of art on their science, were blunt in their denial. We saw that the denial was accompanied by assertions about the nature of science that prize its objectivity and rule-bound behaviour. We are therefore impelled to ask: do not the affirmative answers now obtained from my first and second research questions force the conclusion that there is a contradiction at the heart of this study: that, in short, the transcripts are being interpreted as both affirming and denying the role of artistic practice in the scientific enterprise?

The opportunity to resolve that paradox is the purpose of research question 4. I signalled throughout the literature review, and indeed elsewhere, that in place of simple declarations about the role of art in science, we should be particularly alive to the way texts, and indeed scientists, might manifest tensions and ambivalences about the issue. My various pairings of writers, for example Hafner and Kuhn, and Henderson and Kemp, allowed me not only to explore a variety of views, but also to suggest there could be no settled, final view on the matter of the role of art in science. In particular the question of art's status as a 'cause' of scientific knowledge would depend centrally on the commentator's view of epistemology.

The transcripts put life into this rather obvious point by showing the scientists in the middle of a dynamic process where they struggle to work out for themselves why they ever engaged in an arts collaboration. Indeed, it turns out that the puzzle which originally animated this study, kept its power throughout the project, and formed the central organising principle. That is to say, my surprise at these busy scientists who professed a clear commitment to demarcation, but committed entirely to their arts collaboration, remained a valuable driver of the study. The transcripts reveal that the scientists are as active in eroding their demarcating devices as they are in maintaining them. Thus, if demarcation relies on the use of hypotheses, we find the scientists doubting their actual value. If science is an austere and 'monklike' process, it is also something deeply marked by the constraints of money, timetabling and institutional politics. And though the scientists nod their approval of the scientific method, they are anxious to express their own individuated position within the intellectual and institutional matrix of their profession.

Most particularly it was the way the interviews so often moved into discussions of the scientist's perception of their professional world, and their role in it, that is so provoking. It was through discussions of artistic practice that these scientists signalled their possession of some initiative, some autonomy, within the grinding processes of their department. The scientists affirmed that unlike their colleagues, they retained a holistic perspective. They were good, too, at talking to people outside the department; indeed they implied it was hard to have a decent conversation *within* the department. The scientists were it seemed open to collaboration, perhaps more so than their colleagues. Pressed on the matter, the scientists seemed 'pre-adapted' for the rewarding environment of the art-science collaboration: Mark Miodownik mentioned his experience of the Oxford college system, while Ian Thompson remembered the inspiring worldliness of his brilliant PhD supervisor, Larry Hench. The scientists pointed to these experiences as partly the cause of an unusual 'mindset' that in turn enabled them to do the unusual thing of setting up an arts collaboration. It is a reminder not only that my sampling of scientists was geared around a search for those who would be articulate about their experience, and able to reflect upon its significance. These are scientists who are important advocates for the merits of art-science collaborations, and of funding schemes and policy decisions which make such collaborations possible. Their descriptions of the way they work, and their tacit admissions about the way they would *like* to work, are embedded in conversations about arts collaborations, but clearly refer to a wider set of concerns about the priorities of contemporary scientific culture.

The scientists are saying: "we are not automata, we can push our work in ways our colleagues may not understand". In that process, we can conclude, the arts collaboration is playing an important role. We can conclude too that we learn as much about the culture of science from these collaborations, as we do about art.

Notice that the scientists very frequently describe how problematic the arts collaboration could be. The money was 'peanuts'; artists were alarmingly insecure; colleagues might disapprove; publication was difficult or impossible; and it might be career suicide. Yet the scientists were adamant they wanted to carry on, or would do it again; moreover there seems little evidence from these scientists careers subsequent to the interviews that any harm had been done – quite the contrary. We can accompany the bullish statements in the transcripts about the scientist's commitment to the collaborations with more recent information about departmental promotion. Anthony Bull was clear about the immediate benefit:

> It's [the science-art collaboration] going to allow me to more effectively do research because I have a wider experience. It's very important I think to have wider experiences, rather than always just answering narrow questions that are incremental. (AB:455)

The tone of the following quote suggests he will be stubborn in protecting this 'space':

> But if someone is looking over my shoulder, saying what are you doing, time and motion, all this, you shouldn't be spending time on it, but anyway nobody does so I don't care. (AB:357)

Mark Lythgoe was eloquent about the advantages that accrued to him professionally, in terms of his ability to communicate, and 'project himself', with colleagues. We saw an example earlier where Lythgoe complained about a constrained and formulaic aspect to science, and suggested that he was to a degree rescued from this rigidity by his collaboration with the film maker Andrew Kötting. Not only does Lythgoe say he 'found himself' through the collaboration (ML2: 32), he balances that against an imagined cost to his career, while adding a final, defiant codicil.

> I've definitely made a terrible error of judgement from that point of view [the career, publishing papers]. However there's no way I would have done it any differently. (ML2:25)

In fact, in 2007, Mark Lythgoe was appointed Director of his unit, University College London's Centre for Advanced Biomedical Imaging. In the period this thesis covers, Mark Miodownik has been promoted to Reader (he was Lecturer at the time of interview), as has Anthony Bull. The art-science collaboration is thus not necessarily a career hazard. Nor would we expect them to be, if the intellectual and personal benefits the interviewees report are indeed authentic. We can see the outlines of an argument for using policy decisions to encourage such collaborations.

Ambivalence and tension is everywhere present in these transcripts. While on the one hand the scientists may assert the separability of their work, on the other they are exploring issues to do with the nature of science, or their institution, through a vocabulary in part made available by the arts collaboration. We must conclude that whatever may be the difficulties an arts collaboration brings, these scientists nevertheless embrace them because the scientist identifies them as *vehicles* by which they can explore their own culture, and assert themselves within it. The arts collaboration, for these scientists, takes its value from its capacity to ferment change.

I suggest that it is this aspect of the art-science collaboration that should catch our attention, and be accorded importance. If my conclusions are fair, the art-science collaboration is one means by which a scientist can challenge aspects of their professional culture, and achieve a measure of re-orientation. Moreover, on occasion, their research practice itself can gain from the encounter. In all this it is the arts collaboration's ability to be both ordinary *and* extraordinary that makes it so richly useful to the innovative scientist. If the scientist wishes, the collaboration can occupy ground that is utterly familiar (Wingate and Carnie sitting looking at images). Yet the collaboration can also be the occasion for provoking a scientist into the extraordinary act of thinking about his metaphysical assumptions (Davey thinking about reductionism).

There are other issues to consider. For example, the role of art as an enabling factor in facilitating the public engagement with science and technology (PEST) has frequently been mentioned in this thesis. Without doubt, artistic practice has become a central resource in the science institutions' efforts to raise their public profile. How does this fit with this study's emphasis on the art-science collaboration as a device for (some) scientists to make changes both in their research and their professional culture?

Some indications can be drawn from the transcripts. For example, though we tend to associate PEST with institutional and governmental initiatives, rather than with particular scientists, it was clear with the scientists I interviewed that many of them were attracted by the possibility of encounters with the public. Further work would be needed to make explicit what lies behind that attraction. Yet there is already a clue in the comment by Mark Miodownik, when considering his increased public profile: "You have to watch your ego". We can speculate that, pressed on the question of what might attract a scientist to taking part in a public event, the scientist might assert the importance of an 'informed' public. Given the way so many of the Wellcome-funded arts projects culminate in a public event, one could argue that it is the chance of interacting with the public, rather than with the artist, that forms the principal motive. Public engagement now enjoys a higher profile within the academy. It is noticeable that funding agencies are committed to the importance of their own public relations, expressed through semi-popular magazines such as the EPSRC's *Newsline,* or through support for Science Week. With input from the Wellcome Trust and from the Higher Education Funding Councils, Research Councils UK (RCUK) are funding eight 'Beacons of Public Engagement', launched by the science minister Ian Pearson MP in January 2008. Such initiatives, though important, are not noticeably innovative in the way they portray the advantages of engagement, tending always to see the outcome in terms of 'an informed public'. It is generally harder to perceive in such work what the aims are in terms of the scientists' own work, let alone their professional selves. It is interesting to speculate however that scientists find a very rich set of outcomes when they work with the public, satisfaction at helping create a more informed citizenry being only one. I noted earlier that Tony Holder was intrigued and stimulated when he worked as an assistant for his arts collaborator in the very environment where he normally worked as a scientist.[104] Just as I have argued that there is utility in the art-science collaboration, in the way it conjures practical and intellectual insight in the working scientist, there may well be useful research to be done in evaluating whether the same could be said for the field of public engagement. Certainly it is becoming clear that

[104] See excerpt 110.

there are important points of contact between the processes of the art-science collaboration, and the better known sphere of public engagement.

Clarification is nevertheless needed. It is worth remembering that the projects described in this study are not simply public events. There may be a public aspect; but in interview the scientists offered rich details about the private intellectual space they developed in collaboration with artists, and some scientists went so far as to distance themselves from any motive to deal with the media. For Richard Wingate, talking is one thing, while going on the radio is quite another:

> I love talking about the science, I'd like to communicate it to more people, but the stepping over the communication barrier into being a media scientist is a very dangerous step indeed. (RW1:670)

It seems then that public engagement remains therefore something of a hazard. There is a perceived risk for scientists who include public engagement in their professional life.[105] Thus we should not imagine that the scientist who worries about the consequences of developing an art-science collaboration, can comfort himself with the knowledge that the public event that eventually emerges will earn him credibility, and offset colleagues' scepticism of the project itself. Quite the contrary; the act of working with an artist and the act of engaging with the public sit both on the same side of the coin. Both take the scientist away from the severe but necessary routines of the properly productive research life. We can thus conclude that the scientist who takes his work to the public, like the scientist who starts to work with

[105] Richard Wingate talked of the necessity of 'being very sure of your credibility' before getting involved with the media (RW1:704)

the artist, is asserting his autonomy. Such a scientist, should it be necessary, will develop these projects while fully aware of the suspicion he might meantime encounter. Going against the grain of the daily professional life may be a – somewhat thrilling – opportunity to be welcomed by these scientists, rather than something to be feared.

This study has shown, I believe, that there are reasons to believe that artistic practice has a functional role in the advancement of science. In making that argument, I evoked an understanding of science that finds little reliable distinction between aspects that are empirical, and aspects that are cultural. Yet though these arguments address the thrust of my original queries about contemporary art-science collaborations, I find as I draw this study to a close that other issues claim equal or greater attention, but which must here remain in outline only.

These issues, in short, all concern the question of the scientist's agency in controlling – constructing – his work. I interpreted the ingenuity and perseverance of the scientists in sustaining these collaborations as revealing a strong desire for autonomy and for intellectual creativity in their work. Work at the art-science interface, given a measure of credibility by the intervention of major funders, and articulating with concurrent debates about the public engagement with science, has provided scientists with a ground that simultaneously encourages innovation, while also enhancing the scientist's ability to reflect on his ordinary professional life. That ordinary professional life, it is worth remembering, is driven for most by the demands of competition, of publication, and of the Research Assessment Exercise. The human cost to neophyte scientists has been graphically described by MRC scientist Peter Lawrence when he noted that the cycles of credit used by scientists to propel their career could be grossly unfair. He has likened PhD students to rocket boosters that lift their supervisors high into orbit, but themselves fall back to earth empty and exhausted (Lawrence 2002). Ziman has labelled 'post-academic' the current form of scientific organisation which, he argues, construes collaboration as a way to embrace e-communication, short-term goals, financial and competitive pressures, and an erosion of the distinction between the academic and the industrial (Ziman 1996). The collaborations studied in my investigation, however, are of a different kind. They privilege relationships, professional development, and the growth of the intellect.

The implication of the current study is that some scientists at least are prepared to take risks in a search to re-frame their professional focus, and recover (or protect) something of the intellectual idealism and creative vigour that perhaps motivated their entry into science in the first place. The art-science collaboration has apparently found especial favour with these scientists, and so it behoves the wider community of scientific commentators and policy makers to take such initiatives seriously.

We can now see why funding organisations like the EPSRC, the Wellcome Trust and the Arts Council were right to develop this terrain. In the last 15 years, those working at the art-science interface have developed a richer understanding of science, and an enhanced experience of public engagement. We should remember that those 15 years, especially in the biomedical sciences, have seen steady comment on the way commerce and competitiveness have impacted on the scientific life. Nick Davey's comment, discussed earlier, that science is "...producing huge numbers of stamp-collecting biologist with no ... initiative, no sort of questioning of the world around them",[106] contrasts most vividly with the aspirations of the interviewees discussed in this study.

I have been concerned, in this study, with the more direct consequences of the art-science collaboration on a scientist's work. Yet it seems fair to wonder how broadly such types of working might inform the scientist's attitude and sensibility. While Lawrence (Lawrence 2002) reminds us of the urgent moral issues sitting at the heart of the process of science, a decade of debate on the public engagement of science establishes that the word 'dialogue' remains barely accurate in describing the relationship between scientific research and the lay public (Irwin 2005). The link between the two is that a scientific community able to develop and maintain an equitable and congenial professional environment is also more likely to be able to take meaning from encounters with the public. In science education too, the ability of the research community to respond to the concerns of teachers and students has been rather partial. Attempts to reform the school science curriculum, so as to make scientific understanding as culturally aware as it is technically proficient (Hunt 2006), has attracted criticism from the centre of the science establishment, the Rector of Imperial College (Sykes 2006).

Opportunities are therefore needed to help scientists develop a better sense of how their work sits in the wider society. Policy shifts, educational reforms and ethics reports are each important, but to a working scientist in the middle of his career they remain abstract and distant sounds in an urgently brisk environment. The art-science collaboration however is an example of a process that is far more immediate, and – judging from this study – far more pleasurable. Part of that process, we have seen, involves scientists making an intense

[106] See excerpt 73, p203.

appraisal of the philosophical and cultural background to their work. Such a focus, I have suggested, has practical value. Moreover it is a scrutiny which will allow active scientists to take their place in the moral and political debates that reach into the heart of their profession, and ramify throughout society.

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Appendix I

The interviews

Name	Institution	Date(s) of interview	Time	Code
Mark Miodownik	Kings College London	May 30, 2002 May 23, 2005	70 80	MM1 MM2
Mark Lythgoe	Institute of Child Health	May 31, 2002 February 1, 2005	70 40	ML1 ML2
Mike Page	University of Hertfordshire	July 2, 2002	70	MP
John Tchalenko	Camberwell Art College	October 3, 2002	60	JT
Daniel Glaser	University College London*	November 26, 2002	70	DG
Nick Davey	Imperial College London	March 20, 2003	80	ND
Anthony Bull	Imperial College London	June 19, 2003	60	AB
Richard Wingate	Kings College London	June 30, 2003 January 6, 2005	70 70	RW1 RW2
Tony Holder	Medical Research Council	November 24, 2003	60	TH
Philip Kilner	Imperial College London	December 10, 2003	70	PK
Ian Thompson	Imperial College London	January 6, 2005	60	IT
Anthony Bull	Imperial College London	June 19, 2005	60	AB
Ian Thompson	Imperial College London	January 6, 2005	60	IT
Alf Linney	University College London	June 13, 2005	60	AL
Neil Theise†	Albert Einstein College of Medicine, New York (USA).	July 13, 2005	50	NT
Marilène Oliver**	Royal College of Art	November 16, 2004	60	MO

Phoebe von Held	Theatre director	May 20, 2005	70	PH
Neal White	Ravensbourne College of Design	July 27, 2005	70	NW
Jane Prophet	Freelance artist	July 28, 2005	60	JP

* Daniel Glaser is now (January 2008) a public projects officer at the Wellcome Trust.

** The four artists interviewed are listed separately at the end of this table.

† Interviewed by telephone.

Appendix II

First interview with Richard Wingate (June 30 2003).

This transcript is included to help give a sense of how I structured my interviews, and of the nature of the exchanges.

SW I'm interested in that area of how scientists see this work they're doing with artists; and then later on I'll try and relate it to more conventional ways of defining science, so for example you might have a view about how to organise a scientific experiment, or what it is to be a scientist; and then I might become interested in how you got that view; and then I might get interested in how that view relates to the fact that you work with Andrew. And so this interview will have two parts, one part is very mundane – what do you do as Richard Wingate the scientist here at Guys and Thomas's; and then how you got into the collaboration, how that, what you do actually.

RW Yeah sure, no problem.

SW So I'm going to start off, Richard, by asking a very simple question, which is, what's a typical day?

RD A typical day, Oh well, getting in, having a cup of coffee, and then switch on the computer always first, read emails, that's the bread and butter of science at the moment, is communication, particularly through the world of computers I have to say; we're all computer literate; probably organise experiments as such, which means going to the bench.

SW Which is not in this.

RW No it's directly across the hallway, so it's very quick access between the two, that's probably three days a week on a good week. Quite a lot of the time, I'd say at the moment, it comes and goes, 50 % of the time; a lot of reading; and more than most people on this floor I do a lot of refereeing of scientific papers, so that takes up a lot of time. Sometimes I won't even come in, I'll stay at home and just sit at the kitchen table ploughing through these things. That's the typical day.

SW So you don't necessarily get hamstrung by teaching?

RW Not me no. Because I'm a Wellcome fellow I am actually exempt from any teaching for the moment, although I will probably in about three years time; there's a lot of long-distance planning in academia, as you know.

SW So you're a Wellcome fellow, which would run for about five years?

RW It should run for four years, and I've had an extension, I'm in my sixth year as a Wellcome Fellow, I've always been a Fellow since finishing my PhD, I've just gone from Fellowship to Fellowship and now I've run out basically; the pyramid has become very very sharp at the top, I've reached a level where it's unsustainable, so I'm going to have to become a University Lecturer, which I guess I've just done.

SW You've just become a lecturer?

RW Yes, I've made the transition this summer.

SW Here?

RW Here, or the Institute of Child Health, but I think it's likely to be here. Running alongside the science there's a very intense, academic political game, which you have to be able to play, you have to be able to master it, to a certain extent, to have some control over your destiny.

SW And what about your actual research; for someone who is not a complete expert, what is it you do?

RW It's very simple. I'm just looking at the way neurones, baby brain cells move within the developing brain of a chicken, so looking into chick eggs, at very young embryos, marking different sets of identified cells as they are born. So it's not a random process, you are picking a group of cells that you think are going to be interesting, making them fluoresce usually, using a jellyfish protein which can be put into these cells on a temporary basis, and then filming them as they move across the surface of the frame. That's the basic hard core of the science; there's a lot of molecular trickery to get them to do different things, change different components of the way that they see their environment and sense where they are going.

SW So chemical gradients, signaling.

RW Chemical gradients, signaling, regional boundaries – the brain is divided into different regions – how they identify where they are, and where they are going.

SW So how do you – I've seen the name Ramon y Cajal in relation to you and Andrew; and I was thinking with these neurones, they are living, but where are they?

RW They are living, but they are in a dish. Or they can be, the reason we work on chickens is because you can cut a hole in the shell of a chicken egg, look in at the embryo, possibly deliver genes by different techniques, the genes can be incorporated into a cell temporarily; genes that will make them fluoresce or do something differently; close the egg again, let the embryo grow, look at it actually in the egg when it is doing that, or take the embryo out – they are a very self-contained system, they are cheap and plentiful, and so yeah, always living cells. But at the end of the day you back up this kind of more, spectacular, quite showy science in a way, with a lot of hard grafting, the basis for looking at neuroscience hasn't changed since the days of Ramon y Cajal in 1900, which is a lot of histology, that's basically slicing brain tissue and any organic tissue, staining it in different ways, looking at it under a microscope, and that's the bread and butter of nearly all anatomical science, I guess.

SW So you are interested in the way these cells are migrating and moving. So,how chemical is it?

RW It's pretty, it's pretty.

SW I mean your aim is to see what triggers movement, or where they want to go?

RW It's all of that. Some migration, it's a very difficult thing to see in a static picture; there is no way if you were say take a brain, slice it in half, stain the cells, look at the cell positions, that you'd know which one's are moving, whether there were populations of cell in there which are en route somewhere else; so it's only relatively recently, and really in the last five or six years, five years maybe or less, we've been able to say, right, I'm interested in this region of the brain here, if it's contributing any cells to this other region over here, so we are interested in every aspect of why they are patterned to move; what they sense when they are moving, to give them an idea of where they are going, lots of chemical gradients, how they actually physically translocate, and how does the internal skeleton of a single cell reorganize itself, pull the contents of the cell from A to B – it's completely unknown – what they do at the other end, and in particular at the moment, why they stop, how do they realize that they have reached a destination. And cells are quite complicated things, they've got long, they may have long, actually the one's I'm looking at only have a single extending process but it's quite long, quite complex. It's a leading axon and the cell body follows on behind. And that means different parts of the cell are encountering different environments at different points, and the key is really working out which part of the cell is significant in sensing where it is, the axons have a little growth cone at the tip, which seems to be the active part, the only part that's doing anything, but is that also, is that pulling the cell along, plus sensing where it's going, plus determining where it goes?

SW Very interesting.

RW That's the question I want to pursue. Again, nothing is known about it.

SW When you think about this in the bath, do you think about the whole brain, or communities of cells. Where's your direction, if you like?

RW I think my direction is slightly different from most people's, in that I do actually think of the whole brain, very much so, and slot in the very very particular, very obscure pathways of cells that I'm looking at, into a global model.

SW Why do you say differently from most people?

RW There's different ways of approaching the problem of brain development. The two obvious ones are, I suppose reductionism is a simple way of putting it, versus a kind of top-down, looking maybe holistically. An example of a holistic approach, I don't think people do this in development, maybe, is to do brain scanning. You just look at the whole brain and see what's happening, when you get a certain stimulus. I presume you could do that with a small child, see how its language centres develop as it picked up new skills, and that would be a developmental study in a holistic sense. Reductionism is very prevalent in this particular centre, it reflects the current trend toward molecular neuroscience and molecular science, and this thing called bioinformatics, where you pick on a certain molecule, it could be a gene, it could be the protein product of the gene, a particular thing, and then you see how many processes it's involved with, and your focus is on the gene; that can lead you anywhere, but the reductionist approach really is looking at a series, ends up being a series of biochemical pathways, in an abstract sense, not related to the brain. So why am I unusual? I'm trying to integrate the two things together, and look at the whole hierarchy of processes, maybe from the internal skeleton of a single cell right through to the relationships between multiple populations of cells and how they interact. That's not impossible by any means, but it's more putting the actual work- which is probably very similar to everyone else's, into a context which is larger. I wouldn't claim to be superior in any way, but I think it gives certain advantages, definitely.

SW I find that very interesting. Could you say more about, I completely understand the distinction you are raising there. How do you go about sustaining that direction of your research? You are trying to be non-reductionist, trying to see the wider picture at a time when everything is very biomolecular.

RW You can look into it in different ways. One of the key one's is evolution. Evo-Devo is a catch-phrase, almost a term of abuse in our field, because it's seen as, a bit soft. It's the kind of world where anyone could step into the theory, but there is a hard core of biomolecular, molecular comparative anatomy which is a useful tool. I would never focus on evolution as an end in itself because evolution is essentially comparative anatomy, it's not for us, it's a different kind of evolution, an old fashioned approach, but the comparative anatomy of new molecular tools can actually tell you quite a lot, it reflects back on the structures I'm looking at. If you see a shark brain, and it seems consistently different from a chicken brain in one aspect, and you can spot that little focal difference, you can probably attach a molecular difference to these two organisms now; so the evolutionary context gives you a whole brain context.

SW I would imagine so. In terms of bolstering your emergent properties philosophy, can you do it here?

RW There are a lot of people working here on this floor, on slightly different systems, so in a sense the emergent philosophy gets a bolstering, almost too much could potentially feed into what I'm doing. So I actually have to filter out the amount of cross talk to stay focused. In terms of conferences, scientific communication is a rarefied means of communication; you get very short time slots to present your case; occasionally you get a longer period in which to, again, put on a performance. So it's really just a case of making it entertaining. It does fit, it does work. What also feeds back into it is that I've got quite a lot of interests in different systems within the developing brain. Limbs; I look back on my botany days with a lot of affection; all that feeds back because brain development is a huge subject, uncharted waters. I've said this to Andrew, in terms of science, it's more exploration and cartography rather than necessarily hypothesis-driven science. And that's a big distinction that.

SW Between.

RW Between the brain research, research into brain function and brain development in particular, and say looking at how the kidney works, which has no scope beyond looking how the kidney works. Eventually you find out everything you need to know.

SW So the brain research you do may not be best construed as "hypothesis driven"?

RW Mmm.

SW Do you find within the work you are doing here are divisions between different aspects of your research? In the sense that for example some is more bread-and-butter and pays the bills, and some is more blue skies?

RW The more blue skies stuff is the fancier imaging side. It spices up the bread and butter side. I find both equally satisfying. I'm actually quite a big fan of the bread-and-butter; that's the real stuff, the nitty-gritty. I know that some of the other stuff ... it's not as useful as it could be quite yet, but it's good to develop because it makes people sit up and pay attention. In terms of division .... I'm trying to think of the relationship between hypothesis-driven work, and the exploration side. The exploration is a global approach outlook, and the hypothesis driven experimental side is to tackle individual problems, piecing together a bit of a broader model; that's when you get the more bread-and-butter side. The exploration side is coupled with this kind of idea of "let's think of some great new technique that's going to open up vistas of new research" So there are in parallel, in tandem, both a scientific approach and perhaps methodological approaches being approached side by side. So yeah, bread-and butter, and more fanciful.

SW And you do both?

RW I try, yeah, very much so.

SW Well I have a really sense of how you work and what you do. Let's talk about Andrew and the art. How did that start?

RW .More or less by accident. Andrew came looking for collaborators. I'm trying to think if I've had any experience of this before, I may have talked to a couple of artist friend before about the concept of trying to put some kind of collaborative thing together in a sometimes commercial, sometimes. The money that comes in from the Wellcome Trust means that friends, artist friends get in contact with me.

SW You are talking about the money that comes from the Wellcome Trust in terms of sciart?

RW Yes, Andrew was slightly different. He already had his funding and remit; he came looking for potential collaborators, and I think I was just the person who was the most willing to sit down and talk to him. I can't remember how it started, but we hit it off on a kind of visual level, because a lot of the work I do has a very strong visual impact, always has done. What I did for my PhD was look at fractal-like brain structures, or fractal-looking, among the higher nerve cells – sort of iconic nerve-cell images which have been used in a large number of contexts, always giving them to people to use for different things, and I think that probably grabbed his imagination as well.

SW So you'd noticed that these particular images work at wide interest?

RW Oh yes; it's something, I'm trying to think of how many times they've been used, in lots and lots of different contexts.

SW OK. So what happened? Andrew came looking.

RW. Mmm. And just came and chatted, and came back three or four times and just chatted about the general scheme of work I'd done, actually not specifically about the work but just general concepts about brain development and I realized subsequently that his interest in migration and movement of people paralleled very clearly some brain themes – in particular the brain cells growing out, contacting each other, dying back, retreating, pruning themselves, organizing themselves; I think he saw a lot of potential in that to marry with his own artistic, whatever, theory, credo.

SW He's interested in travel, and.

RW Yeah, travel and meeting. Yeah, if you look back at all his stuff, there is a lot of travel-based work in there.

SW That's what I've picked up from talking to him. How would you manage, what about the technical aspects, when you talk about neurons and signaling.

RW He's got a zoological background, so he's got some of the language, which is a good start for understanding even the fact that the brain is made out of cells. We take that for granted, but it is a relatively new hypothesis. Something I'm quite interested in, that is long forgotten, the cellular hypothesis of the brain is something that happened in the last hundred years – before that it was mush. So in terms of understanding of the basic nuts and bolts of what is going on, half the battle is won. Technically, there wasn't much to explain. It was just a question of giving my view of what is actually happening in the brain, when things are connecting up, and what they are doing, and why brain cells need to do this. What the rationale behind organizing their connections, trimming the populations, re-organising these connections again, what the rationale behind that is. There are lots and lots of different mechanisms that feed into it.

SW So you began having these meetings. Was he already planning *Magic Forest*? I know, *Magic Forest* I know. It was on in the Head On exhibition at the Science Museum. Was that you and him?

RW Yeah. I think that was unusually close, judging by it. I think Andrew would say that. Looking across the spectrum of what other people were doing there, a limited spectrum, that was more of a genuine collaboration.

SW A genuine collaboration

RW A product of a series of conversations, a few sketches, Andrew bringing his drawings in to me, showing me what he was doing, I wasn't checking it or ticking it or giving it marks, but talking through the ideas and the images that he drew for that, are taken from images that I myself drew of brain cells that I had photographed. So he had quite a bit of material, maybe he worked from photographs, I'm not sure, I've got a wealth of, stacks of drawings.

SW So what was he taking,? It is a series of silk screens, there are a series of images moving back and forth. There is a sense of complexity and movement developing.

RW Well in terms of, I don't know what the metaphor, is that the right word, or the plan, the text, whatever the word is, is basically a developing piece of brain tissue, all the cells in it are born, they reach out, touch each other, make contact, become – form this fantastically dense forest, then start reorganizing themselves, becoming I think a regular structure. Some of the cells are lost, the branches become less complicated, and the whole thing essentially could become more ordered. Although I think in Andrew's piece it doesn't really have this sense of becoming more orderly over time. I don't know if that's what he was trying to do, it is not a cycle, it is just a maturation process.

SW There is a strong sense of directional change.

RW Mmm. We were also both very interested in, Andrew's been looking at the, what's the name of the guy, the photographer who analysed locomotion ....originally English, later moved to America, I can't remember his name, took all these endless strip films, Muybridge.

SW Muybridge.

RW Andrew is interested in Muybridge and I do a lot of time lapse filming. Time lapse is an interesting thing. Actually filming is an interesting thing; you are compressing time down – you are skipping a lot, you are missing things. And talking to Andrew ....this was feedback to me from Andrew....The format of the screens – the idea of sections – the idea of time points – and the fading between then on only three pieces of cloth to represent the entire brain is very much a rarification of what we are doing – we really only get glimpses, it's an ethereal thing, very difficult to put your finger on, we never see the entire brain in action, all its components, because it's just too complicated. It cannot be visualised, except in pieces. If you stained every brain cell in a little one millimeter cube, you'd have a block of dark brown material; you couldn't see a single thing. So you have to always go for individuals to represent the population as a whole, the brain as a whole. You take representative individuals. Then if you are looking at development you take representative time points, put them together to make a sequence; and you usually slice up this tissue and take one in a hundred slices, in fact in scientific terms I feel the piece is a bit cleverer than just. we always worried we'd end up with an animation, like a documentary movie of.

SW It evokes this issue of slicing.

RW It evokes the issue of slicing, time slicing and real slicing, and has something of the methodology in it as well as showing something of the process. It's quite, I think it's almost an experiment on Andrew's part, and I think he's pleased with the way it works out; but having those components in it made me think about what I was doing and.

SW So tell me about that. How does *Magic Forest* sit with you when you think about your work?

RW It's quite satisfying because having spent thousands of hours drawing these cells, and thinking they are amazing things, to think that there is a representation at least of "brain" out there, in *Magic Forest*, which is getting closer to what it really looks like, which most people don't ever see. I think textbooks are particularly appalling in their representation. If something is too complicated, it is always easier to schematically strip it down to a blob. In fact that's another thing they did. There are many many ways of representing brain structure, from at worst, I'm talking about anatomy here rather than processes, a wiring diagram, or a couple of blobs with interconnected – this bit connects to this bit. At best some people have come up with an intermediate kind of diagram that shows some representation of the organization of a particular brain region plus its connections. It's very difficult to do, to build an accurate, it is an impression, it's never going to be. I suppose the key is, is the impression meaningful? Are you conveying a meaningful part of that brain structure. And perhaps the most accurate would be to try and again draw every cell in, squish them all back together again, you'd end up with a black piece of paper again, because it's just too complicated. So it's finding that intermediate part, while retaining a sense of the beauty of it, and showing what it is – very difficult to do. I think that's what was satisfying about *Magic Forest*, it showed some of the beauty of it, without actually … it didn't lose, yeah, it showed some of the beauty of the complexity of it, I don't think there is any other representation of it. I was hoping it might have surprised a few people, looking at it and thinking "wow, is that what the brain feels like?"

SW A few scientists, or a few ordinary people?

RW Ordinary people. Ordinary folk. I hope it did.

SW It's a very beautiful work of art.

RW I think it works very well. In terms of thinking back on what I do, it definitely made me think about the models, the way of presenting things, particularly the model-making aspect. Actually the conversations with Andrew were very interesting, in terms of just refining my ideas, and it definitely wasn't a one way process, it definitely fed back, I think Andrew actually came up with one or two good ideas, good points, I'd go away thinking, yeah that's a good aspect I hadn't thought about, because, as you started out asking about what I do, a lot of boundaries, stop points, start points, reasons for doing things, are the language that Andrew is dealing with in art. And so if I've missed out something, like "oh well this cell goes this way, round here, turns right, turns left and he says "Just a second, didn't you just say it does a U-turn there, why does it do that?", he spots something, there's nothing to stop him from spotting something that I would have missed.

SW There's a sense in which that's almost technical. He might ask a question about direction, or say that doesn't sound right because you said it was, through his own interest in spatial aspects he could see a contradiction you had come up with?

RW It takes a very strong three dimensional spatial awareness to deal with, I find it very natural and very easy to think in three dimensions and rotate objects in my head but I realize when I'm explaining the structures that I'm looking at to other people, they just don't get it. That also helps, talking to someone who's also got a very clear three dimensional view – you can sketch out a schematic and Andrew can see the form in it very quickly and very clearly. It's very useful.

SW Going back a bit, it sounds as though you have got a strong visual imagination. Do you see that in yourself?

RW Yes, I really recognize that as being quite an important part of what I do, and it actually frustrates me that when I think of an adult brain, this sounds ridiculous, I know the name of the nucleus, say the dorsal lateral geniculate nucleus is one which does bug me actually, and I can't quite see, the brain starts off as a tube, just a tube, and topologically it is still a tube when it is completely grown, a canal runs all the way through; but the entire convoluted form of our brain is really just a twisting and forming of the walls of this tube as it thickens. And if I can't quite picture where on that initial tube the dorsal lateral geniculate nucleus comes from, I find it incredibly, just really, I want that whole brain model of something evolving from a tube into the complex shape that we've got inside our heads, I find it really....

SW You want to fix it?

RW I would like to see it as an animation I understand.

SW I was interested, you said you know with the brain you can't have every single cell there.

RW No, absolutely.

SW There have got to be gaps. In science, are we trying to understand everything, or are we trying to get a composite picture which is meaningful. And I was wondering, as regards *Magic Forest*, how it feeds into your way of thinking about the brain. You talked about being reductionist and thinking about it holistically. I was wondering how you relate that work with Andrew to the directions you're research goes in.

RW In terms of....

SW In terms of your desires almost. Where am I going as a research scientist in terms of the brain.

RW It's interesting that, I don't think many people look ahead more than, I know quite a few scientists who find it difficult to look ahead beyond the next small experiment, and actually get quite nervous about long term prospects. Pretty much the most successful scientists in my field are the one's who've had a broader view, particularly technically. I've always been interested in the imaging side, of seeing brain cells of my work, and probably working with Andrew has reinforced that a little bit; but it has made me think, particularly, about the partialness of it, that we are getting partial pictures. And putting that back that back together again, the schematic diagram is the model, there is nothing better, that's all, that what you end, that is the end goal, the best schematic model. You can't have a complete representation, you can have something that is way too simplified. So you are aiming towards something that gives as much meaning as possible, which you can then hang a lot of data on, specific data. Specific data which will cure a disease, or help you understand cancer. The model is quite important, and it's evolving and changing. This is something I've been thinking about a bit with Andrew, or independently from Andrew. I can't say it's directly resulted from conversations with Andrew, but I think it's been triggered by that.

SW *Magic Forest* is not a complete picture.

RW No.

SW But there's something quite authentic about it. Certainly as a work of art, and it sounds as though it's saying something about.

RW It's not inaccurate, it's not an untrue piece of art. It's not something that fed off science and used it's images to make something aesthetic. It's actually got a meaning within its structure that relates back to where it came from, if that makes sense. It actually is a structural representation in it's structure it is a representation of the way that those images were acquired in the first place. The whole thing is a complete piece, in itself. I don't know if Andrew would see it that way, but that's the way it would seem to me. I don't know what other scientists would say, everyone who's been along has said it's "very nice", but I don't know if it had any meaning for them, I'd like to think it did.

SW I remember reading, I was also very keen on Cajal, and this extraordinary idea that the stain would pick up some things and not others, and I hadn't realized that this was still a matter of interest.

RW There's no-one better yet, and there's no one who has spent so much time or effort as Cajal. He also used his imagination quite a bit to reconstruct and get that intermediate schematic model. That was the enormous interpretative strength that he had over other people at the time in the field who were using the same techniques, they would describe cells but they couldn't make sense of it, and he made a lot of sense. The remarkable thing is that after 90 years is that nothing he guessed at has been proved wrong. He was right on every score, practically, apart from details, and he probably, there's not a single field in developmental neurobiology that he didn't touch on or comment on, so he was quite extraordinary but he was not a typical scientist, he was an anatomist, he was a thinker who worked with very very basic bread-and-butter materials, rather than a scientist who analytically uncovered the secret of the structure of DNA.

SW He was an anatomist, he wasn't doing experiments as such, he wanted to build up a picture.

RW Yes, he was collating together a lot of, I don't think he ever did anything particularly experimental. We can do experiments now that would understand some of the structures he saw, I don't think he could have done, but he anticipated the experiments by suggesting that this structure looks like it's the receiving end of a cell, this structure here look like it's the growing sensing end. He sometimes overshadows everybody else who was around at the time, that's more to do with just how accurate, how good he was, rather than the way he's been interpreted, or because people have just forgotten about every one else. That's one reassuring thing about science in a way; if it's good it's promulgated, if it's bad science it's forgotten.

SW What about realism? Your pictures of the brain, your structures you are building up, how do you view them in terms of the truth of what the brain is like?

RW They've always got to be models, even if you, it is Heisenberg's uncertaintly principle, when you look at something your disturb it, this is certainly very true of cellular, when you are looking at a brain, there is so much you have to do to it first before you can actually see it, you are never going to get an accurate, truly accurate representation of it, although you can get quite close. That's actually changing, it's probably going to be possible to get very good movies in the near future, two or three years, of brain cells maybe deep within the brain, at cellular level doing something.

So far that's been impossible. Technically that's something that's changing very rapidly.

SW I asked Andrew this question. When you worked with Richard, and you were getting information from Richard about neurons, what was he, Andrew , interested in terms of the final product and even the science, and he said that he did have ideas about the science but reckoned that the amount of time he'd have to spend reading and studying he'd have to do, in order to input on that level would be great. But he did say this thing you mentioned too, that he was very interested in migration and travel. I was just wondering, when you are having your talks, and you perceive him as an artist with his own preoccupations, I know you talked about this already, but things like his interest in migration or travel or exile, how do those ideas , do they just swim around? Do you feel as a collaborator that you have to take certain ideas of his? What do you need to get from Andrew in order to feel that the collaboration is a collaboration?

RW I think it's the, I wouldn't expect Andrew to come up with an experiment, though he could do, he'd be more than welcome, certainly could contribute to it, a lot of it is just logic, what should you be looking at; I think it's more the context in which it's done, it's difficult, what would it be? The context of the science? I'm sorry I'm trying to think.

SW It's very broad.

RW I was trying to think if the word was "philosophical". I think his contribution has made me pause – I was always interested – but more actively, is this something that people should be more aware of, the kind of frailties of, yeah the frailties of what we are doing. There is a lot of, what would be the word, I've got to stop for the word! The interpretation that we have is based on staining patterns and methods of analysis that again are,to go back to that point, partial truths. And particularly with Andrew's interest in Ramon Y Cajal looking back at the origins of our concept of brain and nerve cells is something really that Andrew has stimulated more. I've always been aware and interested in the history of neuroscience, but I'm always quite shocked, with the point I raised earlier, that less than 100 years ago, the brain, the mind, the brain-mind was considered as something other, it had no relationship to the cells of your skin or blood cells; it was known for many hundreds of years that the body is a cellular structure, but the brain had always existed separately as, something different, they are not quite sure what it is. There are so many of us now working in neuroscience, and it's almost as if we, as each new generation comes in, the history of it is buried very rapidly. There is only a 10 year, 8 year, five year now historical component of people's work, when really an 80 year, a broader perspective is actually incredibly valuable. A long roundabout thing would be to say that the perspective aspect is really important. I've been thinking more about that I suppose. Even with Andrew talking about Muybridge, the origins of time-lapse photography are again 80 years ago, and once again we take, it's partial truths, it's snap shots; and in the latest thing that Andrew is doing, he's talking a lot about the nature I guess of the snapshot, as opposed to a continuous feed, if there is such a thing, a continuous feed of images, which there isn't I guess, it's always time lapse of a sort.

SW So he made you more, him as a trained artist, you might not have heard of Muybridge before, he's made you more aware.

RW I wouldn't say he's introduced me to any new information. I did know about Muybridge before, but actually talking about his concept of image and, maybe not his themes, going back to your original question about migration , I don't think the themes are particularly useful, because they are emotional themes and scientifically it doesn't mean anything, it just doesn't mean anything. The actual technical approach and the way we see. It's more the way we see. That's what it comes down to.

SW OK. When you mention the word frailty, what were you referring to?

RW I suppose, it's not fallibility. Fallibility would be to imply that the thing you were looking at was incorrect. It's really just the frailty being, yeah the holes, if I was looking at you I'd see your outline, the colour of your shirt, maybe get some sense of where you are, but that's it. That's the level of the information we are dealing with, and not really the complete picture that a lot of us would probably like to think, unconsciously think, when we stain for one particular molecule, that we unconsciously think that we are getting a picture of how the brain works.

SW So this is really a comment of yours on what you think about scientific knowledge?

RW Knowledge in general I don't , certainly I do see, It's difficult to know about scientific knowledge about all fields, there's such an enormous variation in what we define as. I'm not sure that the thing I do is strictly speaking science all the time. I like that idea of exploration, I certainly see my particular field as being colouring in; if I look at my mental model of what a brain looks like, of how much we know, there are big uncharted areas and small parts which are intensely characterized; but across the whole of science? I've got some friends who are mathematicians and, again that's not really science, I don't know where the science comes in, there's mathematics on one end, there's exploration at this end, and some point in the middle there's someone somewhere probably doing a real experiment. But I sometimes wonder whether the actual science is something that's almost a second-level process that can be utilized to find out certain things. You want to find out why a rain forest tree isn't doing particularly well because there's a coal plant next door, you pour ammonia on it - which I've done, out in Borneo, you measure its leaves, and then you say "ah well, this ammonia is not too good for this rainforest tree". That's the scientific process, but I'm not sure it's science in the sense of searching for knowledge. There's certainly two ways of looking at science, one is process and one is, it's not even the mission as a whole; science is just what human beings do, they like knowing about things.

SW So this is a debate about whether those technical skills about doing experiments, whether that exhausts what we mean by "science", or whether the exploratory moments you have are also science?

RW Certainly I think there are a lot of folk who work at the process level, and do good science as a result. Good science involves taking into account all of the potential factors, and coming up with an unbiased judgement; I use those skills as well, I have them modeled on the broader science in which to put them. Probably the interaction between the two is sometimes productive, sometimes not, because the broader knowledge can, you can squeeze results into any framework you want, you've just got to be careful you don't over-squeeze them.

SW I'm almost through! But I've got a third strand which will take less time, but it's to do with that broader knowledge you mentioned. *Magic Forest* is quite an important instillation, it's discussed, it was at the Head On gallery, it'll go around probably; you had to write stuff about it; how do you relate that part of Richard Wingate - the bit which has a collaboration funded by Wellcome, relate that bit back to you, is it important to you, what's it for?

RW It's very important. I really enjoy it. I enjoy talking to Andrew more than I enjoy talking to other scientists about what I do, that's for sure. That's a key thing, actually, that it's more interesting trying to explain what I like about science, than having a scientific chat, which is vaguely competitive and sometimes a pretty useless process. In terms of ...is it different, is it the same, the two things flow together, Andrew has his meetings here, he's helped out in the lab, I don't draw a distinction between the two aspects of what I do. There is a very interesting part about it though, which is – it's a part I don't talk about to other scientists at all, it devalues the scientific credibility enormously in my mind, though maybe that's a subjective opinion, to be up there in the sciart world; the culture of science is the hermetic, or hermit, ascetic, who does his monkly experiments and produces fine, crystal clear science. And that's my goal as well. I love writing a super-sparse, beautiful crafted paper which has no extra frills in it at all. That doesn't exclude the sciart part; but the sciart part. I wouldn't ever want to get drawn into it, or have my, I don't want to become a sci-artist. And that's been quite interesting, realizing that there is this prejudice surrounding the whole thing, stepping off the narrow scientific path is a very risky thing. So there was a certain amount of risk in what I did with Andrew, although it's not something I'd pull back from. Although, having said that, I probably have pulled back a bit, I wasn't really keen on being too heavily associated with it, I wouldn't want to fight the cause of sci-art. It's also been very interesting seeing the way the art world works as well, going up for sci-art prizes, and hearing, the two worlds exist in parallel, and have very similar structures to my mind. And hearing a committee of artists evaluate our work is one of the funniest things I've ever heard. The sci-art committee, it was just hysterical. The level at which the work is discussed is kind of, irrelevant in the science world. It made you quite glad to retreat to science where you are judged on your ideas and how effective they are. It's not quite, I can't quite put my finger on it, what it was in the sciart interview, it was more, more.

SW More conscious, more intense?

RW More feeling, conscious perhaps. I know a couple of scientists, a lot of scientists have an artistic side, and think "I could do both, it would be so easy, in my spare time I'll paint, I could be as good". But I've got a lot more respect for say Andrew's credibility, he's got an equal credibility to me in his world, and it's earned in very similar ways and the divergence is quite huge actually. The language spoken there is so humorous and comic, "what are you talking about?", I thought I was able to converse and answer questions in any field, but this was just like, another world. And it did make me think back to this concept of Renaissance figures. I got phoned up by someone who was doing a programme about Leonardo's brain, asking why are there no Leonardo da Vinci's, was it because his brain was extraordinarily big or something like that? So I wrote a thing back saying "wise up and go with the 21st century". But it's more that, It's not that no-one is capable of being a good artist and a good scientist, but more that the worlds are so completely different, you can't exist in both, you really can't, there's no way that as a scientist I can have credibility while also exhibiting art at the Royal Academy. And as a Royal Academician, well you just can't, there's not even a question, the era of the Renaissance man was way back, and that's the only time it could have happened. These strange articles, "where are the Renaissance men or women of today?" well it's not possible, the worlds have completely shifted. So that's one of the interesting things about Andrew's collaboration, I've got a glimpse into that.

SW Going back to that question of tiptoeing out of Guys, you mentioned the degree of suspicion amongst colleagues, you mentioned that this is not the sort of thing you want to pursue too heavily because, there's.

RW Oh yes, worrying.

SW I wanted to ask first of all how this is manifested, but also to what extent is this something inside you?

RW I think it's very largely something inside me, and it's based on the reaction I have to other people who would get too involved in peripheral activities. It happens a lot with media scientists. There's a couple who I respect, who I know, and there's a couple who I know who are useless scientists. You think: "what right have they got to be talking to the media?" That's really strange, because that implies that you've got to be, only the top of the field should represent our cause, but that's in a sense the way credibility, you are almost getting, if you divert into sciart, you are getting your credibility too cheaply, not paying for it in the normal way of building up a, you are not allowed to have extra credibility given to you by other people outside the field. It's purely subjective, no one has every come up to me and said: "You sci-artist!". In fact most people come up and say, that's really good, well done, nice...".

SW So it's not literally the institution, you've got the freedom to do it.

RW Yes. No-one has ever, I've kept a low profile in Kings College, who would be very happy to know more about it, I'm not going to go around trumpeting it.

SW You've kept a reasonably tight hold on it?

RW I didn't tell anyone about Head On. They just found out about it as it went along. I'm very happy to apply for as many sciart grants as possible, and I'd love to do more with Andrew, but I'm not going to, it's not going to be, Andrew can come in here and he can spend as much time talking to me as I want, but it's probably not going to be something I want to post up. Although there is a small value in it, in that it does get people, the stray people wandering past, interested, wandering past on the internet, wandering past down the corridor looking at your poster of your work. It does catch people's interest. Just at that basic level of noticing the work that you do, be it potential postgraduate students, that's usually the best one actually, you can attract more interesting ...

SW They notice...

RW The broader approach is indicated by the fact that, hey, I can...

SW An undergraduate or postgraduate could know this work, they could spot it on the web-site?

RW Probably on the web-site, if I posted it up there, which I probably will. It might attract a broader-thinking person, so it's got some utility. The monkish aspect of being a scientist, though, is I think quite, there is at the core of it, the essential part of it, I suppose in science you live or die by your public published work. In that respect, whether you do sciart stuff till you are blue in the face it doesn't matter, it is your publications that count. That really does encourage this kind of, the producing work for objective assessment, independent of personality. Maybe you should be personality-less, I don't know.

SW I suppose this might have changed too; you wanted a sense of history in terms of neurophysiology and the brain; I suppose there is a history of what it is like to be a scientist, in terms of how important the RAE is; things have changed a bit.

RW Yep, It has changed. The purer you are as a scientist, again this is subjective, the better it is. I have no problem about talking, I love talking about the science, I'd like to communicate it to more people, but the stepping over the communication barrier into being a media scientist is a very dangerous step indeed. Which is sad. I think you are either one thing or another at the moment; maybe it will be possible at some point in the future to dip in and dip out at any level of science, but really – you must know this, at the top you've got Susan Greenfield and Colin Blakemore and Lewis Wolpert, all three I've known or worked with at some point. They have translated into being the people you pick up the phone and ask about "The Brain", and you've got to be very sure of your credibility before you do that because of the three of them, Lewis and Colin are giants in their field. Susan, she's not absolutely a top scientist, and so she's really, it's a perilous thing that she's done.

SW A lot of what Lewis does is simply offer an opinion on, you know, science and art.

RW Yeah, he's got to that stage. He has the credibility to be able to offer his opinion, but I don't have the credibility to offer my opinion. Sciart is an interesting outlet for that, but I think, actually could it lead you along the route you've pursued, and maybe do humanities, I can see how that would happen: you suddenly become history of science, or history of neuroscience, overnight. It's something I'm dodging a little bit at the moment, I have been asked to write a history of neuroscience piece; and I think – how much time is it worth investing in this?

RW Having said, well, you don't really want to be up there associated with the whole effort, I was quite surprised at how little, in a sciart sense, how little attention is paid to the scientist.. I think in terms of Head On, we could have been easily have been forgotten. There was certainly no thought of seeing what element of the science impinged on the work. It's a one way appropriation as I'm sure a lot of people are saying at the moment. There's a debate at the Old Operating Theatre coming up, which is "The Appropriation of Science". That concerns me a bit. The point would be: maybe I would have liked to have had more say in it, because the appropriation idea gives the impression that we are a lot of dumb guys in white coats sitting around handing out, "you want the leg of a foetus, sure take it, photograph it by all means" this whole Alder Hey scandal that we have no broader feeling for our work than a technical one, is in some vague way reinforced by the idea that in these science-art collaborations we are the anonymous donors who just offer up material and ideas for creative artists to exploit. I went through a phase, a couple of weeks, of being quite angry about this, thinking "this is ridiculous". And this would overcome any of these feelings that I must be the austere monk; and I'm sure it is something that needs to be corrected. And any opportunity at a science-art, they have these various spurious get togethers, I raise my hands and say you really are not interested, the sciart people are interested in the art. The Wellcome Trust are very keen on their wine and cheese pre-exhibition receptions, they love it, they love that whole world, but they are not really thinking very hard about what they are trying to do.

SW So when you get angry, there's some cage-rattling going on, this is something that you think is important?

RW It's not that we are being done down; I think people are being misrepresented in a slight, It just adds a log to the fire of misrepresentation, of science being vaguely irresponsible, slightly sinister; it's the anonymity side of it which I was saying before

I should encourage, stay away from being too clearly associated, that's not quite right. If I could stand up there and say look, this is the interface and the contribution the science has made to this piece of art, and for it not to be, I don't want to be in the Observer, and people to come in and read about it on Monday morning here in the lab; if the lab world could be separate, I would do it, but to be tagged as a science-artist in the science world is very bad. But outside, it is actually quite useful.

SW So you don't want the scientist to be seen just as this provider?

RW Yes. It does happen that way; I'm sure a lot of the science-art collaborations have been a mining of resources, which is a bit sad. That's not what was intended originally, but I don't think anyone is really making a, the people who are distributing the money, and after all it is a financial exercise largely, are really not keeping a close check on what's actually, so in the science-art committee there wasn't really anyone who had a clear idea of, it seemed to me, among those who contended, I think we did a good thing, a good piece, but we didn't win the top prize, and those who did were kind of quirky pieces: let's send an artist to the Antarctic, let's track artists' eye movements with these eye-trackers, and its like ... what? Why? What's the interest in that, that's a scientific, I mean it's interesting, but it's not a science and art meeting in any way at all, in a productive way. There's no meeting. Science is a thinking, philosophical exercise; art is a thinking, philosophical exercise as well in some respects, although I really respect the more emotional, feeling side of it. I think it's just incredible that you can martial that and produce a piece of art, because I don't think I could. And that's really ignored, it's a shame.

SW So what's ignored is the idea that science as a thinking philosophical attitude could in some way learn or be influenced by.

RW Yeah, the two could rub something of each other, because the ways of seeing is the key, except in a technical process of sticking an eye-tracker on a, that's literal science, I'm not even sure what the philosophical context of that scientific study is, although there are, I mean looking at gaze control, there are interesting problems there as well, which could rub off in an artistic collaboration. I would rather have seen some of the thinking about the concept of steady gaze, what does it mean to always, despite all the movement around you, to have this completely, absolute crystalline steadied precise view of the world; and take that aspect of it, rather than say hey, a painter uses his eye differently, lets take a look at this. I thought this was extremely disappointing, but another winning entry in the sciart competition. So, yeah, I would be a champion for there is a deeper level of interaction, but it is not easy to do.

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